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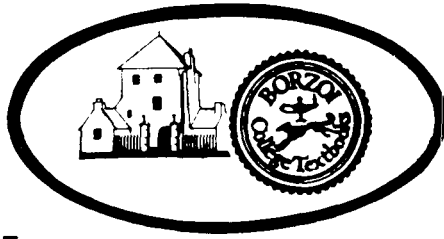
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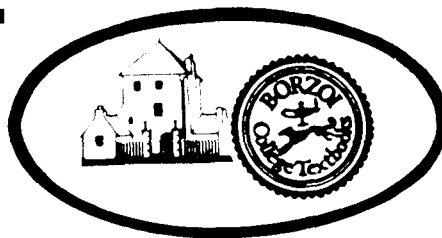
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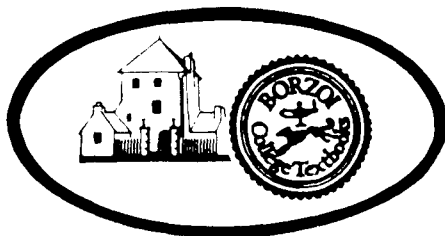
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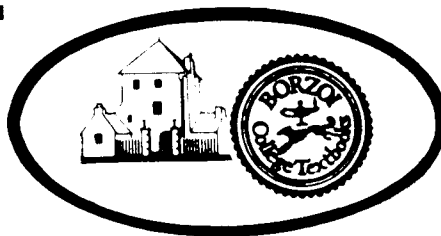
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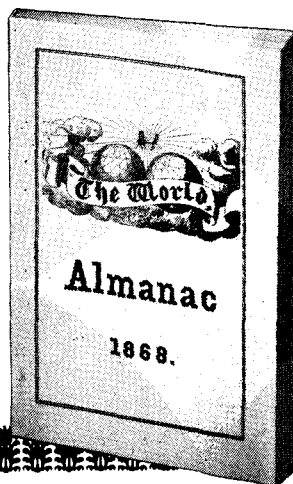
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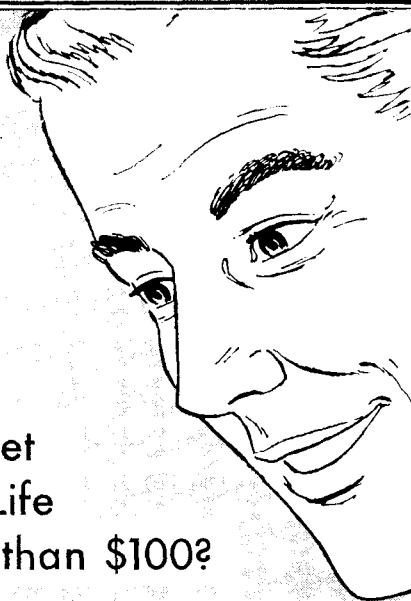
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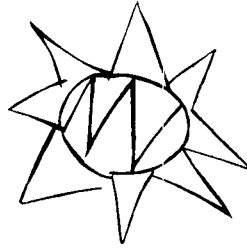
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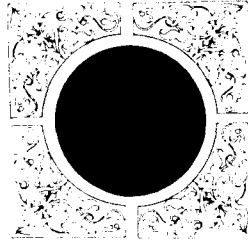
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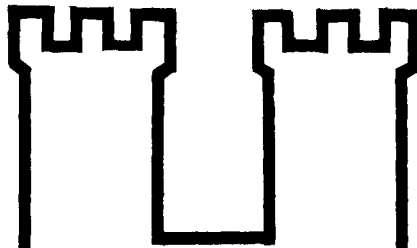
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FEBRUARY 1968

The History of Ideas

Hajo HOLBORN*

PROBABLY almost everyone attending the Annual Meetings of the American Historical Association, and even more this year's combined North American meeting, will derive encouragement from seeing historical scholarship actively represented by such a strong professional corps. As a group it seems to possess the capacity of knowing most of the events in six thousand years of human history, while it is devoted to filling the pages of history that for one reason or another have so far remained blank. But at the same time many of us will feel apprehensive about the course of development that our historical studies have been taking. The programs of our meetings clearly demonstrate the continuous growth of specialization and, with it, the danger of the fragmentation of historical scholarship.

In spite of the eminent position that history holds in general education, historical thought probably does not exercise the same strong influence on the formation of the philosophy of life among our intellectual leaders as was true fifty years ago. The specialization of historiographical interests is at least partly responsible for this decline. There are some fundamental questions that historians are unable to answer satisfactorily at present.

*Mr. Holborn, Sterling Professor of History at Yale University, delivered this presidential address at the Royal York Hotel, Toronto, Canada, December 29, 1967.

Among the most important problems I would count the construction of universal history. Ultimately all historical study will have to be related to the understanding of universal history from which it will receive its sense of final direction. The practical value of a clear conception of universal history for a generation witnessing the meeting of all cultures in global interaction is obvious. The general demand for a model of universal history has led the public again and again to give stormy applause to such theories as Oswald Spengler or Arnold Toynbee presented. But they have kindled only straw fires.

The problem of universal history is, however, only the extreme task whose solution is impeded by the specialization of historical scholarship. Twentieth-century historians deal, actually, in their own work only with small slices of universal history, only, as a matter of fact, with small sections of units of history, mostly circumscribed by nations and epochs. To this division according to subject matter, methodological differences may be added as they have come into existence, as, for example, those between political, economic, and intellectual history. But, although few would deny that division of historical studies often assumes excessive forms, its inevitability cannot be seriously questioned. Once historians accepted the principle that true history could only be written from the original contemporary sources, the needed research rose to gigantic proportions, and the division of labor became ineluctable.

The demand for the writing of history from contemporary sources was an expression of the belief that this was the only way to perceive the real past in all its uniqueness and individuality. Many historians and philosophers feel that the historian's characteristic task is exclusively the study of the individual facts of history.¹ If taken literally, this would lead, and has led, to the assumption that history consists of a huge mass of mutually unrelated facts to be established by independent research. Hand in hand with such an assumption goes the opinion that facts exist outside the mind of the observer and consequently can be simply perceived. They do not call for the judgment of the historian, but, on the contrary, they have to be cleansed of all "subjectivity" that he might bring to them.

These theories, which if strictly applied would produce only the dry bones of history, have often been refuted. Here it may be briefly stated that historical facts, which are dead facts, come to life again only in the mind of the historian and are part of a universal development in which we ourselves partake. A historian who writes and lectures, which means he is using

¹ Thus in particular the Neo-Kantian school in Germany; see R. G. Collingwood, *The Idea of History* (Oxford, Eng., 1946), 165 ff.

words, cannot help placing the individual event into a larger scheme because language renders the particular in generalizing terms. What we admire in great historians, moreover, is their capacity for relating the individual to the universal, whereby they reveal to us its full historical meaning. Yet it remains true that without the knowledge of the huge variety of individual phenomena history cannot be reproduced and evaluated. But while even the small units reflect something of the totality of human history, through the compartmentalization of historical studies, the historian's vision stays considerably narrowed.

We ought to inquire whether there are approaches to history that can extend our view. The history of ideas or intellectual history, which came into existence relatively late and has gained a place in the American academic curriculum only during the last thirty years, has aroused great expectations, and some of its practitioners even today impress me as being convinced they possess a special key to the inner workings of the historical process. Yet the history of ideas has not achieved a clear and generally accepted definition.

Voltaire was the first, in his *Essai sur les mœurs*, to declare the history of the human mind to be the historian's real subject. Political history is only the tale of external change, whereas in religion, philosophy, science, and art the human mind passes through various stages toward its present state. Voltaire did not deny that politics had an important part in the rise of human culture; international pacification and the establishment of a firm legal order within a country were for him the foundations of flourishing civilizations. But the rise of the individual to higher self-realization according to his rational nature constituted the true contents of history. Voltaire was not able to make his conception of history prevail for long. He believed that man had remained essentially the same throughout history and that only custom, prejudice, and outward circumstances had hidden and obstructed the power of reason. This belief created a paradox. As Ernst Cassirer expressed it, if the substance of the human mind is immutable, it has no real history.²

Hegel followed to some extent in Voltaire's footsteps, while at the same time going far beyond and away from him. The idea of progress was also to Hegel a natural concept in looking at history, and spirit was its real life. But spirit in Hegel's philosophy of history was something quite different from Voltaire's *esprit*. To Hegel history is the unfolding of the universal mind in this world. It occurs when man has risen beyond the state of nature and has begun to use his human potential—thought and

² Ernst Cassirer, *Die Philosophie der Aufklärung* (Tübingen, 1932), 288 ff.

reason—which gives him the power to change reality. The rational character of man realizes itself only in the community of men. The state, Hegel says, is the embodiment of morality and links the individual with the great historical development of history. In the state the “objective mind” comes to life. The forms of the state reflect the stage of awareness that man has achieved of the power of reason or of his own freedom. For the “essence of mind is freedom,” the freedom that derives from knowing the world as its property.³

Hegel strongly emphasizes—perhaps more so than many modern historians—that man is also part of nature and that human passions and appetites are the great incentives of action. But behind the cravings and the struggles of the individuals whose consciousness is impaired by their personal passions, the universal mind moves forward to higher levels. Even the “world-historic” persons, such as Alexander, Caesar, or Napoleon, were driven in their actions by personal interests, but they lived at a moment of history when the existing system of values, laws, and rights was ready to yield to the realization of as yet unfulfilled potentialities; by overthrowing the existing order these great men helped to create a new age. They were not the real initiators of progress but only the agents of the world mind. Hegel speaks of this condition as the “cunning of reason.”

Thus history is dominated by the world mind. Though building the world of the objective mind, man is only a laboring puppet. Yet he gains greater freedom as he becomes conscious of the freedom toward which the course of history is directed. Religion, art, and science—for Hegel this meant philosophy—surpass the state. They are forms not of the “objective” but of the “absolute mind,” and they enable the individual to know his connection with the universal mind. The dynamic power behind the phenomenal world, however, is still reason, which determines the development of history in accordance with its own laws, the laws of logic. Here, then, Hegel introduces dialectics, the famous sequence of thesis, antithesis, and synthesis.

Only a few of Hegel’s key ideas have been sketched here in order to demonstrate that in his view history is a universal process in which each stage has to be judged as an integral part of the whole. But since the really historic is only what is rational, the panlogism of Hegel’s philosophy makes ideas the chief objects of historical study. They are the essential achievements of mankind, while at the same time they demarcate the consecutive epochs of history. Hegelian philosophy was for this reason stimulating to the writing of the history of ideas on a large scale. The Tübingen school

³ G. F. W. Hegel, *The Philosophy of History*, tr. J. Sibree (New York, 1899), 17.

of Ferdinand Christian Baur applied Hegelian principles to the study of Christian religion, and one of the members of the school, Eduard Zeller, with his book on *The History of Greek Philosophy*⁴—that was considered important and useful enough to justify in recent years a new American edition—carried Hegelian concepts into the history of philosophy. Hegelian influence can still be traced in the modern history of ideas or, for that matter, in general historiography, perhaps most clearly shown in the circumscription of the epochs of history.

But the appeal of Hegel's system faded quickly after his death. In Germany the so-called "Historical School" and Ranke constructed a history in which, to Ranke, "every epoch was equally close to God." This did not allow one to impose on history an a priori pattern or to see in history the foreordained course of reason. In contrast to Hegel's monism, Ranke believed in a transcendental God, although he thought that occasionally the historian might discover God's hand in historical events. At the same time, Ranke embraced the totality of human life, including much of what Hegel called "fortuitous existence" (*faule Existenz*), as the true subject of history. And he was anxious by inductive method not only to reconstruct the mere events but to elucidate their meaning in a universal development. The often quoted remark from the foreword to his first work, that he "only wanted to know what actually happened," is bound to be misunderstood if taken out of context. The statement was merely a denial of the intention to present lessons from the past for the present. Although it is true that Ranke in the fifty-odd volumes of his writings concentrated largely on political history, much is found in them on social history as well, particularly in his *English History*, and the author of the *Roman Popes* and of the *German History in the Age of the Reformation* always had an eye on religion.⁵

In Western Europe Auguste Comte, Herbert Spencer, John Stuart Mill, and others elaborated the "positivistic" philosophy, which was an even more direct continuation of the thought of the Enlightenment than the Hegelian philosophy had been. What Positivism eventually amounted to was the assumption that facts could be immediately ascertained by sense impression and that historical laws could be formulated inductively analogous to the laws of the natural sciences, of which progress remained the most important one. Positivism appeared in many different forms. It did not, in general, emphasize ideas as strongly as Hegel or even Ranke had

⁴ On Baur and Zeller, see Wilhelm Dilthey, *Gesammelte Schriften*, IV (Leipzig, 1921), 403-50.

⁵ See Hajo Holborn, "The Science of History," in *The Interpretation of History*, ed. Joseph R. Strayer (Princeton, N. J., 1943), 59 ff.

done; it attempted to counterbalance the study of the individual by the examination of the group and society. After the middle of the nineteenth century, Positivism also exercised great influence in Germany, although the German historians, at least in theory, rejected the idea of progress as well as sociology. Hegel and any metaphysical philosophy of history were, on the other hand, declared to be unscientific, and Ranke's students quickly forgot the universalistic ideas of their teacher.

It was in the 1860's that Wilhelm Dilthey began his work as a philosopher and historian. More than any other scholar he was the father of the modern history of ideas. It was a philosophical interest that led Dilthey to history. He objected to the subordination of the humane studies or cultural sciences to the natural sciences, as was common among the Positivists. Even Immanuel Kant had based his theory of knowledge on physics and had not given history and the humanities any recognition as sciences. Dilthey intended to set beside Kant's *Critique of Pure Reason* a *Critique of Historical Reason*, designed to establish the cultural sciences on a secure, scientific basis.

But in contrast to the contemporary German academic philosophers, many of whom attempted to revive Kant, Dilthey felt that philosophy had to part with "logism," the belief that, above the reality, eternal regulating principles exist. In "the spirit of the great Enlightenment," he always intended to retain "the empirical reality as the one world of our knowledge";⁶ in this respect he also never denied a kinship with the Positivists. Dilthey maintained that philosophy could not go back beyond life. "Life is prior to knowledge."⁷ But there is a world of the spirit, which man creates in time through his feeling, willing, and thinking. Man tends to organize his general understanding of what appears to him as his world in a *Weltbild*, that is, world picture, and his own reaction to what the world ought to be or to become in a *Weltanschauung*, or world view. World pictures and world views are the chief sources of our knowledge of the human mind. They are the product of the "living experience" (*Erlebnis*), which we are able to re-experience by an act of empathetic intuition.

For Dilthey the study of the history of ideas was the way to achieve a valid philosophy. It is important that his concept of mind is much more comprehensive than that of Hegel. While Dilthey, too, sees in philosophy the highest capacity of the human spirit, he assigns to the poet probably an even greater creative role, and beside the poetic vision stands the practical will power that finds expression in the laws of the state and society. Phi-

⁶ Wilhelm Dilthey, *Gesammelte Schriften*, V (Leipzig, 1924), 418.

⁷ *Ibid.*, VIII (Leipzig, 1931), 264.

losophy rests largely on the utterances of human imagination and volition and, by abstraction, lifts them to a higher rational level.

Dilthey was the greatest historian of ideas, and although the modern practitioner of the history of ideas often may not be aware of the fact, his influence in this field has been extraordinary. Dilthey's admirable studies, which fill many volumes of his writings, were, however, written with a philosophical intent. They were to him only building stones with which he planned to erect a new system of philosophy. But, if he wanted to extract a philosophy from life or history, he had to solve a major philosophical problem. He himself defined it once in the following words: "Life is given to us not immediately, but elucidated through the objectivation of thought. If the objective conception of life is not to become dubious by the fact that it passes through the operations of thought, the objective validity of thinking will have to be demonstrated."⁸ This goal he never reached in his long life.

Dilthey was a philosopher of stature. Although Ortega y Gasset's statement that he was the "most important philosopher in the second half of the nineteenth century"⁹ goes too far, Dilthey's philosophy is not only significant as a parallel to William James's pragmatism and to Henri Bergson but also as adumbrating ideas that affected phenomenology, psychoanalysis, and existentialism. Yet Dilthey did not succeed in completing his philosophical system. His studies of the history of Western thought confronted him with a great variety of mutually incompatible world views. Originally he thought that they could be reduced and unified by psychology, and, when physical psychology failed him, he developed a psychology as a branch of the cultural sciences, the so-called descriptive psychology in contrast to the causalistic psychology. But this did not lead him philosophically much further; nor did his last attempts to formulate a typology of philosophical world views establish a common foundation. Dilthey in his last years occasionally tried to comfort himself by pointing out that the universality of the historical consciousness frees man from the limitations of his station and makes him understand the varieties of world views as expressions of human potentialities, thereby challenging his creativity by raising "the activity of man beyond the limitations of the moment and place."¹⁰

But obviously philosophy would do better if it provided man with a definite world view and a clear set of values in order to enable him to act in the confidence that in his time and place he was representing universal

⁸ *Ibid.*, V, 5.

⁹ José Ortega y Gasset, *Concord and Liberty*, tr. Helene Weyl (New York, 1946), 131.

¹⁰ Dilthey, *Gesammelte Schriften*, V, 338.

principles. Dilthey, however, admitted with sorrow that the history of ideas leads to relativism, to the "anarchy of world views."¹¹

Yet we are more concerned with Dilthey's history of ideas than with his philosophy,¹² although for a moment we shall eventually have to return to the latter. Dilthey's history of ideas has added a new dimension to historiography by expanding it to include, apart from the rational thoughts, the imaginative visions and the conative efforts of man. Not only conflicting systems of philosophy of a period could now be shown to represent various expressions of a common living experience, but the visions of artists and the motivating ideas of statesmen could also be related to the same experience. The spirit of an age, which Hegel and Western Positivism characterized only with naked ideas, could be described in its many-faceted and dynamic life.

At the same time, the historical characterization gained in precision. Jakob Burckhardt, in his classic work on *The Civilization of the Renaissance in Italy* (1860), had described the outlook of the "first-born modern man," the Italian of that age, in terms such as "individualism" and "realism." Dilthey, a great admirer of the Swiss historian and sage, took exception to these general categories, which were applicable to other centuries as well. In his own "Conception and Analysis of Man in the 15th and 16th Centuries" Dilthey gave a more exact and objective delineation of the Renaissance mind.¹³

It is true that Dilthey's studies on the history of the human mind centered very largely around philosophy, literature, poetry, and music and that he himself did not deal with the visual arts any more than he touched upon political and social ideas. But these were personal rather than philosophical limitations, and others have rounded out the fields to which Dilthey did not directly contribute. Thus Max Dvořák and many historians of art, such as Erwin Panofsky, applied the methods of the history of ideas to the visual arts, and Friedrich Meinecke did the same for political history.

Historians have become willing to accept the analytical findings of the art historians with regard to the artistic styles of various historical periods as adequate descriptions of periods of general history. The term "The Gothic Age" or the "Age of the Baroque" has frequently been used. This

¹¹ *Ibid.*, VIII, 198.

¹² I have dealt at greater length with Dilthey's philosophy in my article "Dilthey's Critique of Historical Reason," *Journal of the History of Ideas*, XI (Jan. 1950), 93 ff. For a full bibliography, the following should be added: Gerhard Masur, "Dilthey and the History of Ideas," *ibid.*, XIII (Jan. 1952), 94 ff., and the section on Dilthey in *Prophets of Yesterday* (New York, 1961); in addition, Carlo Antoni, *From History to Sociology* (Detroit, 1959), and Wilhelm Dilthey, *Pattern and Meaning in History*, ed. H. P. Rickman (New York, 1962).

¹³ Dilthey, *Gesammelte Schriften*, II (Leipzig, 1921), 1 ff.

makes sense only if one assumes that the artistic style of an age is the projection of an ideal disposition of historical man that determines his actions and reactions equally in all other departments of life. Does not such a view also presuppose that mind or spirit is the ultimate force behind the whole development of history? I suspect that many historians of ideas assume this to be the case and that they are attracted to the study of ideas because it seems to promise the revelation of the innermost causes of historical development. In this form the expectation is not justified. The history of ideas cannot be isolated to the extent that this was done by Dilthey.

Dilthey, as has been said before, wanted to purge the philosophy of history of the "logism" or intellectualism of the Hegelian type. This meant two things: on the one hand, the removal of metaphysics and the demand not to go beyond life itself; on the other, the inclusion of art and religion in a system of world views that superseded the exclusive emphasis on philosophy and science as in Hegel and Comte. But actually Dilthey did not go the whole way. Life in Dilthey's philosophy was identified exclusively with the living experience of the individuals and individual groups that have formulated religions and philosophic ideas or paradigmatic aesthetic forms. This narrowing down to what appears in the consciousness of creative men is far from a consideration of the totality of life. He himself knew that a great reality exists beyond the inward experience of the individual, but he maintains that it is given to us only in symbols, signs, and so forth.¹⁴

Dilthey was aware that man is a man among men and that as a social being he is conditioned by the society of which he is a member. In one of his articles in which he describes the historical origins of modern science from ancient Greece and Israel to René Descartes and G. W. von Leibniz, he wrote:

The long Middle Ages . . . beginning with the fourteenth century turned toward their end; in the labor of thinking the individual had won his freedom. At the same time a decisive change of the economic life and the social orders of Europe took place, and this resulted in a total shift of intellectual interests. The work of the bourgeois classes in industry and commerce appeared as an independent force in the midst of the feudal and ecclesiastical orders of life. It directed the mind toward this world. Thinking probed into nature and man. The significance of reality and the autonomous value of family, work, and state were felt and recognized.¹⁵

Yet, although these sentences clearly state a causal connection between ideas and social and economic facts, Dilthey and most of his successors never attempted to establish specific relations between social developments

¹⁴ *Ibid.*, VIII, 225.

¹⁵ *Ibid.*, III (Leipzig, 1927), 8.

and particular ideas. Thus the sketchy description of the changing social conditions of an age remained more of a painted backdrop of the stage instead of being used to explain the drama itself. Only indirectly does the social structure of a civilization, for Dilthey, affect the progress of the mind that is realized through the life experience of individuals. As an example we may quote the words with which Dilthey in his brilliant study of "The Eighteenth Century and the Historical World" introduces Voltaire. After outlining the political reasons that gave rise to the great culture of England in that age he says: "Voltaire absorbed this ideal of a powerful but free order of society when he came to England. Yet since he was with every fiber a Frenchman, he clung firmly to the independent value of the culture of his joyful fatherland."¹⁸

No doubt color, and for that matter historical color, is added to the picture of the evolution of ideas by such methods, but no immediate links are created between the social reality and a system of ideas. Actually, however, world views are always created as answers to practical human needs and not only as abstract syntheses. To be sure, the philosopher—and something similar may be said of the poet or artist—may be interested in a study of past philosophical systems without reference to their concrete origins. He wants to study them as paradigmatic thought models with a view to getting inspiration from them. Behind this is the belief in the existence of what Leibniz called the *philosophia perennis*. To the historian, however, this approach is not adequate.

The historian ought to be cognizant of the nature of ideas. The individual forms a life picture in which his position vis-à-vis his fellow men and society is an integral part. He uses ideas to construct a world view that serves not only his self-understanding but also the determination of his attitude to society, which may be reactionary, conservative, reformist, or revolutionary, and this attitude may change within a changing society or the shift of the social status of the individual. Seldom are these fully conscious choices, and the ideas are rarely original, but usually follow a group pattern. Even the man with novel ideas and visions is hardly above the group because he addresses a public that he tries to persuade or possibly spur into action. We must therefore conclude that social history is the necessary complement to the history of ideas. As Ernst Troeltsch stated, without living up to his own advice:

The rise and development of such theories cannot be detached from the concrete needs and interests of their contemporary environment. . . . They are intellectual structures that cannot be torn from the practical needs and circumstances that

¹⁸ *Ibid.*, 226.

create them. They are therefore justified not only from the point of pure theory but mainly from that of their practical contributions and effects.¹⁷

It seems doubtful to me whether the philosopher Troeltsch did not go too far in this statement by placing the emphasis exclusively on the effects of ideas. Crane Brinton, too, has perhaps unduly narrowed the scope of the history of ideas by saying that it is interested in "the relations between the ideas of the philosophers, the intellectuals, the thinkers, and the actual way of living of the millions who carry the tasks of civilization."¹⁸ I would still believe that even the study of ideas that had no significant influence can be justified if it illustrates the horizons of thinking in the period of their appearance. There are, moreover, ideas that begin to have an impact very late. Arthur Schopenhauer's chief work, *The World as Will and Idea*, appeared in 1819 and received practically no attention. But after 1850 it began to be eagerly read and had a tremendous effect for the rest of the century.

The history of Schopenhauer's influence cannot be explained in terms of the logical unfolding of ideas. It can be understood only through social history. The German intellectuals of 1820 saw in German idealism, and particularly in Hegel's philosophy, the most satisfactory world view in accordance with their living experiences,¹⁹ whereas the generation that had lost the Revolution of 1848-1849 and lived in the dawn of the Industrial Revolution turned to Schopenhauer. On an international level the reception of Søren Kierkegaard is comparable. The philosophy of the Danish thinker, who died in 1855, was revived in Germany after World War I, in England in the 1930's, and in America after World War II.

For the explanation of the continued actuality of truncated systems of thought we have again to take recourse in social history. Leaving Marxism alone, little remained alive of Hegel's system after 1840 except the glorification of the state. Hegel's teaching that there is no secular judge of the conflicts of states and that the decision on which state represents the higher historic principle can ultimately be found only in war appealed strongly to the supporters of the new imperialism. Yet a Heinrich von Treitschke had not forgotten that in Hegel's words the state is the embodiment of morality and that religion, art, and science are forms of the absolute spirit. As a consequence, government by law was to Treitschke as much a natural ideal as the toleration and furtherance of free religion, art, and science. Since Hegelianism has been so often called by German Nazi

¹⁷ Ernst Troeltsch, *Deutscher Geist und Westeuropa* (Tübingen, 1925), 19.

¹⁸ Crane Brinton, *Ideas and Men* (New York, 1950), 7.

¹⁹ See Hajo Holborn, "Der deutsche Idealismus in sozialgeschichtlicher Beleuchtung," *Historische Zeitschrift*, CLXXIV (Oct. 1952), 359-84.

and anti-Nazi professors as well as by non-German writers a forerunner of National Socialism, it should be pointed out that there is no ideological bridge between the two.²⁰ National Socialism was absolutely opposed to any universal principle, to free religion, art, and science, and also to government by law. As a matter of fact, to the Nazis the state was not the highest community but rather the *Volks*, and, of course, theirs was not a philosophy of mind but a biological materialism. Hegel's destruction of the philosophical basis of international law may be named a contributing factor not to National Socialism but to its acceptance and support by members of the traditional academic intelligentsia, and this again cannot be judged to be an event exclusively in the history of ideas.

In insisting on the need for social history in conjunction with the history of ideas I do not suggest that the development of ideas ought to be interpreted economically, least of all in the Marxian sense which requires that ideas be made the mere superstructures of the economically conditioned class conflict. Marxism is not improved as a theory of history if it is deprived of its dialectics and its revolutionary prophecy, as is being done by many American scholars who seem to be satisfied to prove that a man just veils his bourgeois sentiment behind ideas labeled objective notions. This ideological debunking seems to me rather dull sport for a historian.

The fundamental difficulty of Marxism is its inadequate picture of the structure of Western society in the various ages. The three classes—feudal, bourgeois, and proletarian—with which Marx operates, although in his strictly historical writings he uses more refined definitions, barely suffice to explain the social movements of an industrializing or industrial society. They are quite inappropriate to the preindustrial society in which social status is affected by many factors other than property, whether landed or financial. The burgher of the fourteenth to the sixteenth century, for example, is hardly comparable to the bourgeois of the industrial society.

It is true, however, that the work for a livelihood and the desire for a secure life are conscious experiences everyone has, and they are bound to affect every world view. There are, undoubtedly, world views that are nothing but structures for the defense or advancement of particular social interests. But not all world views are that narrow. In using ideas the validity of thought has to be defended, and this requires the proof of its universality and autonomy. Over many centuries we observe in Western civilization the growth, by no means even growth, of rationality. The High Middle Ages exhibited such development. In contrast to the East, the West saw no

²⁰ This thesis has been ably proven by Herbert Marcuse, *Reason and Revolution* (2d ed., New York, 1954).

caesaropapism, and the spiritual principle received protection against the political powers from the Roman Church. The Church also nourished a theology in alliance with philosophy. In the development of the medieval cities, groups of merchants and artisans went outside the feudal system, bent upon organizing and administering their business along more rational lines. About the progress toward greater rationality in modern times, it is not necessary to speak here. With it the capacity for the rational control of human affairs has been growing. This control, however, is precarious and constantly imperiled by passions and interests to which the human mind remains forever tied.

There is a continuous interaction between mind, on the one hand, and interest and passions, on the other. The latter two, desiring power, want to make the spirit their servant in order to objectify themselves in ideas. The mind on its part, in order to realize itself, tries to tame interests and passions and force them to allow for the realization of the potentialities of man. The assertion of abstract notions is of no avail in this struggle. The human mind has to take sides without, however, giving up its freedom, which is its true essence if never fully achieved. As a matter of fact, some of the worst dangers that beset the life of the mind stem from the nature of ideas. They tend to become rigid and strongly resistant to replacement by new ideas more fit to reflect novel conditions. But the human mind, in spite of temporary stagnation and grave reverses, has shown itself a decisive force in building civilizations and thereby has not only survived but also gained in strength.

It is the task of history to recognize man in time. Only through history are we able to transcend the limitations of our own station in time and space and become aware of our full potentialities. But this requires placing man in the midst of his total social environment, from which we shall learn about his civilizing strength and weakness. Aiming at the highest historical truth we shall fortify our courage to be free.

Some Recent Directions in American Cultural History

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THIS essay is not a historiographical survey; nor does it propose a daring new method that promises to eliminate the confusion surrounding American cultural history. Rather, it is an attempt to distinguish three levels of abstraction or specificity that characterize some of the best recent approaches to the subject. The purpose of such an approach is to call attention to fruitful developments in theory and method and to emphasize the need for a convergence of the three levels if we are to integrate separate insights and perspectives. Finally, by reference to my own experience I hope to suggest how the cultural dimension may enhance our understanding of the more conventional history of social controversy, decision, and action.

Cultural history should, it is generally agreed, mean something more than the history of the refinement and corruption of taste. Books offering chapters on the history of literature, architecture, sculpture, and the graphic arts, followed by a condescending bow to "popular culture," are not sufficient. For at least a full generation historians have been more or less influenced by the anthropologist's definition of culture as all learned behavior, or, more precisely, as "patterns, explicit and implicit, of and for behavior, acquired and transmitted by symbols, constituting the distinctive achievement of human groups, including their embodiments in artifacts. . . ."¹ This concept of culture embraces not only the thematic patterns of art and religion but the structure of language, the character of science and philosophy, and even modal personality types.² Anthropologists, for their part, have acknowledged that culture is "historically created" or, in Ralph Linton's

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¹ The definition given by A. L. Kroeber and Clyde Kluckhohn, quoted in Clyde Kluckhohn, *Culture and Behavior*, ed. Richard Kluckhohn (New York, 1962), 73.

² I follow Alex Inkeles in referring to "modal personality" structures, that is, the averages toward which different groups in a society tend to converge, in order to avoid the highly controversial concept of national or cultural character. (See Alex Inkeles, "Some Observations on Culture and Personality Studies," in *Personality in Nature, Society, and Culture*, ed. Clyde Kluckhohn and Henry A. Murray [2d rev. ed., New York, 1956], 577-90.) For the theory that cultural value systems are internalized within the psychological structure of individuals, see *ibid.*; Florence R. Kluckhohn and Fred L. Strodtbeck, *Variations in Value Orientations* (Evanston, Ill., 1961); *Toward a General Theory of Action*, ed. Talcott Parsons and Edward A. Shils (Cambridge, Mass., 1952).

words, "the precipitate of history." In a sense, then, all history is cultural history, and objects once dismissed as insignificant except for antiquarian collectors may now be soberly scrutinized as "cultural artifacts."

The concept of culture as a distinctive way of life leads to what I would identify as the first and most abstract level of cultural history, which can be defined as a description of the characteristic styles, motifs, and patterns of a given period. This is what Jacques Barzun had in mind in his persuasive essay, "Cultural History as a Synthesis,"³ which sets the goal of conveying the "intelligibility of the whole" by elucidating the relationship of the parts. Although evidence may be gathered from diverse and seemingly contradictory artifacts and modes of expression, the aim is to uncover unifying tendencies and common sources of interest and concern. Whether the historian selects his material from poems, public orations, pieces of furniture, or scientific treatises, he must ultimately rely, according to Barzun, on what Blaise Pascal termed an *esprit de finesse*—"the gift," in Barzun's words, "of seeing a quantity of fine points in a given relation without ever being able to demonstrate it." One should add that the cultural historian must supplement his *esprit de finesse* with expert aid from specialists in the history of science, religion, literature, and art. But both anthropology and traditional art history furnish successful examples of Barzun's method and indicate that the unique texture and inner feel of a culture cannot be reached by quantitative sampling and content analysis.

The very comprehensiveness of the anthropologist's definition, nevertheless, threatens to engorge the historian with indigestible detail. The first broad level of cultural history raises formidable problems of selection and representativeness that are not resolved by Barzun's theory of a creative elite from whom the masses derive their synthetic culture. Many recent writers, among them Carl Bode, R. W. B. Lewis, and Howard Mumford Jones, agree with Barzun that the student of culture must find a middle ground between the anthropologist's and the older literary historian's concepts of culture.⁴ Yet this vague middle space provides no theoretical criteria for establishing the configurations of ideas, styles, emotions, and values that fascinate these historians. We may agree with Jones, for example, that American culture was produced by an interplay of Old World projections and New World adaptations and modifications, but this does not provide a plausible connection between his discussions of the Renais-

³ In *The Varieties of History, from Voltaire to the Present*, ed. Fritz Stern (New York, 1956), 387-402.

⁴ Carl Bode, *The Anatomy of American Popular Culture, 1840-1861* (Berkeley, Calif., 1959); R. W. B. Lewis, *The American Adam: Innocence, Tragedy and Tradition in the Nineteenth Century* (Chicago, 1955); Howard Mumford Jones, *O Strange New World. American Culture: The Formative Years* (New York, 1964).

sance man and the American landscape. Whether such historians limit their attention to the most "articulate thinkers and conscious artists," as does Lewis, or choose popularity as a standard, as does Bode, there is often a disturbing arbitrariness to both the selection and grouping of data.

It needs to be stressed, however, that the cultural historian who searches for unifying patterns is not pretending to explain the total functioning of a social system. He may confine his attention to a formal analysis of underlying similarities in religious and aesthetic concepts, as in Arthur O. Lovejoy's pioneering essay, "The Parallel of Deism and Classicism," which is surely a kind of cultural history as well as a history of ideas.⁵ He may narrow his focus to the emotional, intellectual, and moral attitudes of a particular class, as in Walter E. Houghton's *The Victorian Frame of Mind* (New Haven, Conn., 1957). Or he may point to connecting ligaments between formal thought and popular culture, as in John Higham's essay, "The Reorientation of American Culture in the 1890's."⁶

At their best, such studies recapture the vital nuances of a state of mind and convey a sense of the functional interrelationship of beliefs, values, fears, aspirations, and emotional commitments. If largely limited to the literary culture of the printed word, they clarify the changing meaning of terms and uncover the family of associations implicit in a given symbol. They take into account not only the avowed principles of a people but what Linton has termed the implicit culture, which may include repressed values as well as premises that are simply unrecognized or taken for granted. As Richard E. Sykes has recently pointed out, moreover, the avowed pattern of a small minority group may lead the investigator to a "masked pattern" in the larger culture.⁷ It must be confessed, however, that historians have been relatively insensitive to both distinctions and relationships between cultures and subcultures. Closer acquaintance with recent studies like those of the five "Rimrock cultures" in the Southwest may bring greater caution to historical generalizations about what "Americans" believed or thought.⁸

Most historical studies of general cultural patterns have assumed a certain autonomy in the imaginative and symbolic life. At this abstract level historical continuity or international influence often appears more significant than a particular political or social context. There have been

⁵ In Arthur O. Lovejoy, *Essays in the History of Ideas* (New York, 1960), 78-98.

⁶ In *The Origins of Modern Consciousness*, ed. John Weiss (Detroit, 1965), 25-48.

⁷ Richard E. Sykes, "American Studies and the Concept of Culture: A Theory and Method," *American Quarterly*, XV (Summer Suppl., 1963), 263. Sykes carefully distinguishes between the various meanings of "masked culture," but adopts, in my view, a too inclusive view of culture.

⁸ See esp. Kluckhohn and Strodtbeck, *Variations in Value Orientations*.

experiments, of course, in relating cultural patterns to social structure, and, for American historians, Alexis de Tocqueville has long provided a remarkably suggestive model. We still know very little, nevertheless, about the relation between cultural styles and such factors as density of population, division of labor, class structure, or the ecology of villages, suburbs, or cities. We are still largely ignorant of the relations between culture and mobility, which should be one of the most obvious targets of inquiry. Despite all we have heard about immigration, the frontier, and the "M" or mobility factor in American history, we know little about how physical and social mobility may have shaped the basic forms and categories of American thought, or how the culture itself may have stylized such activities as withdrawal, regrouping, and assimilation.

Barzun expressed a fairly common assumption when he defined style as a pose or stance adopted to meet the common needs of an age: "Style is the solvent in which incompatibles are meant to merge." There are dangers, as Clyde Kluckhohn has argued, in all such utilitarian definitions of culture, since cultures may create needs as well as provide means for fulfilling them, and adjustments to new needs may be hindered by survivals from earlier adaptations.⁹ But whenever cultural histories have moved beyond description and classification, they have assumed some functional model of cultural forms. In other words, the patterns or motifs of a culture have been seen as meeting some need for preservation, adaptation, assimilation, or reconciliation.

This brings us to the second level of approach which, instead of tracing broad patterns of unity, focuses attention on some central problem or antinomy within a culture or segment of culture, such as a literary or religious tradition. Such a narrowing of scope would seem to be consistent with the anthropological theory of Kluckhohn and A. L. Kroeber, who identify the "essential core" of culture as historically derived ideas "and especially their attached values."¹⁰ If this cultural core were chosen as the central subject matter of cultural history, the social historian would presumably be left with the actual functioning of social systems—customs, classes, institutions, and social roles—except as these aspects of a total way of life reflect a pattern of "ideas and their attached values."¹¹

⁹ Kluckhohn, *Culture and Behavior*, ed. Kluckhohn, 27.

¹⁰ *Ibid.*, 73.

¹¹ I am aware that this concept of cultural history intrudes upon the domain of intellectual history. There are some intellectual historians who are primarily interested in the derivation, influence, and intrinsic quality of clearly articulated ideas. But as other intellectual historians have widened their subject to include not only ideas in the cognitive sense but values, norms, tacit assumptions, ideologies, images, and myths, they have staked out a claim on precisely the ground defined by anthropologists as the essential core of culture. It seems pointless to debate whether studies of this nature should be classified as intellectual or cultural history.

There are two quite different strategies for moving from broad categories like Romanticism or Victorianism to specific cultural response and function. The first is to discern how a culture frames attitudes toward some fairly universal problem such as poverty, crime, war, or slavery. This is probably most fruitful when two or more cultures or subcultures are compared. The second approach, which has been the main thrust of the American studies movement, is to expose a contradiction or cluster of tensions embedded within the culture itself as the result of an interplay between past choices and commitments and new ideas or situations.

The concept of American culture as a dialectical struggle or debate between contradictory forces is largely of literary origin. One thinks of Van Wyck Brooks's simplistic division of high-brow and low-brow, Constance Rourke's sensitive recording of contrapuntal themes in folklore and literature, and Lionel Trilling's view of American literature as the embodiment of cultural contradictions. The dialectical theory of culture also draws theoretical support, however, from recent work in psychology and anthropology. Leon Festinger, for example, has held that, while every human decision or judgment resolves a conflict between two or more opposing cognitions, a decision also creates a "dissonance" with the remaining cognitions of rejected alternatives. Much of our mental life, both individually and socially, consists of efforts to reinforce decisions, to reduce "cognitive dissonance" generated by new experience, and to establish consonance among coexisting cognitive elements, and between our cognitions and reality. If human action always necessitates dissonance and incongruity, these are themselves powerful forces affecting behavior and attitude.¹² On the anthropological level, Cora Du Bois has found "spurious" conflicts between such American values as conformity and individual success, which derive from an otherwise harmonious commitment to equality, perfectibility, and man's mastery over nature. She suggests that when Americans adopt the posture of friendly, informal "good guys," they are trying to reduce tension built up by the apparent opposition of achievement and conformity. Florence Kluckhohn has argued that strains created by the dominant value system give rise to variant patterns which, though originally part of the system's self-maintenance, are themselves a source of innovation.¹³

Since the publication of Henry Nash Smith's *Virgin Land* (Cambridge, Mass., 1950), students of American culture have been increasingly alert to

¹² Leon Festinger, *A Theory of Cognitive Dissonance* (Evanston, Ill., 1957). For criticisms and alternatives to the dissonance theory, see Roger Brown, *Social Psychology* (New York, 1965), 549-608.

¹³ Cora Du Bois, "The Dominant Value Profile of American Culture," *American Anthropologist*, LVII (Dec. 1955), 1232-39; Kluckhohn and Strodtbeck, *Variations in Value Orientations*, 43-45.

the ways symbols and myths may conceal or temporarily accommodate conflicts in belief and value. Smith himself demonstrated how American responses to the West reflected a whole network of cultural antinomies and conflicting expectations. Thus the West was at once the "passage to India," the route to imperial expansion and power, and a refuge, sanctified by nature, from the complexities and corruptions of Europe; the western hero, personified in Daniel Boone and Leatherstocking,¹⁴ was at once a pioneer subduing the wilderness and a natural aristocrat whose virtues depended on his communion with primitive nature and his freedom from human society; the American heartland was both a bountiful garden that would supply the world with food and wealth and a self-contained retreat that protected the simple virtues of agrarian democracy and provided a safety valve for the dangerous pressures of the urbanizing East. Despite increasing conflicts between the myths and realities of the West, Smith found that the mythology continued to influence public policy and to shape the collective image of the American character.

Charles L. Sanford, in *The Quest for Paradise* (Urbana, Ill., 1961), suggested that the American dualisms of expansion and escape, of millennial mission and primitivistic withdrawal, were ultimately derived from biological impulses, similar to Sigmund Freud's Eros and Thanatos, which led to self-assertion followed by regression to an infantile Nirvana. The dualism was, historically, a secularization of the traditional Christian mythology of redemption, in which the West came to stand symbolically for a material paradise, and the East for hell. A similar dichotomy emerged from Lewis' account of the literary and philosophic "dialogue" over the meaning of America's newness. The fact that America had long been seen as a new beginning for mankind, as a revocation of the past, drastically altered the whole scale of values with which the literary imagination had interpreted human experience. The collective myth of the American as a new Adam, as a man of innocence who is free from the limitations of past decisions, forced the literary mind to search for new symbols and a new design that could help account for the actual complexities of American life.

Despite the brilliant dialectical skill often displayed in such studies, many readers have wondered, with some justification, what precisely is being discussed. Above all, they have asked, what relevance do literary images and symbols have to the concrete events of history? Given the conventions of literary tradition and the contrivances employed by writers

¹⁴ The hazards of forcing fictional characters into a preconceived theory of American culture are illustrated by David W. Noble's refutation of the idea of Leatherstocking as an idealized New World Adam. (See his "Cooper, Leatherstocking, and the Death of the American Adam," *American Quarterly*, XVI [Fall 1964], 419-31.)

to create illusions and arouse the interest of readers, how can one make inferences from literature to the culture as a whole?

The answers to such questions have usually been so vague and unsatisfactory that it is well to look briefly at two books that are relatively successful in relating cultural symbols to historical events. Leo Marx, in *The Machine in the Garden* (New York, 1964), takes pains to avoid confusing literary values with the attitudes of various elites or the population at large. He argues, however, that the pastoral ideal, a complex of images and values long embodied in European literature, shaped early attitudes toward the New World as a middle ground between primitive nature and urban civilization. As the old "imaginary landscape of reconciliation" was adapted to fit the concrete facts of the New World, the pastoral ideal became assimilated to the rhetoric of politicians and the collective consciousness of the people. In other words, what had once been a literary convention for harmonizing values became a shared mode of ordering meaning and value in a context of rapid social and economic change. Because technological progress could be imaginatively dissociated from urban Europe and adjusted to the middle landscape of American democratic pastoralism, Americans could picture themselves as pursuing rural felicity while being dedicated to wealth, power, and production; they could think of themselves as immune from the force of history while leading the world in historical progress. Confronted by an event unique in history—the penetration of the rural landscape by the machine—Americans avoided a sense of conflict by appealing to the pastoral metaphors of reconciliation. By adopting what Marx terms the rhetoric of "the technological sublime," popular leaders like Daniel Webster made industry appear as part of nature.

And yet the sudden arrival of the machine created an elemental dissonance to which imaginative writers were forced to respond. The tradition of European pastoralism had included not only an idealization of bucolic serenity but a larger literary design which, by acknowledging the threatening force of either urban power or untamed nature, introduced elements of irony and ambivalence. Drawing upon this literary tradition, American writers were able to explore the endless layers of significance compressed within the image of the machine in paradise. The machine could suggest, for example, the smoke and fire of hell or the mechanization of life and thought. Since they worked with metaphors deeply implanted in the culture and interwoven in public discourse, writers were able to articulate tensions and conflicts at the heart of their society and yet masked by conventional rhetoric. Marx shows, then, that literary conceptions could become models for public policy, that the impact of rapid industrialization could create deep

strains in the system of values, and that a novel like *Moby Dick*, which was in no way typical of American fiction or of American social values, could illuminate the inner contradictions that lay at the very core of American culture.

Although William R. Taylor's *Cavalier and Yankee* (New York, 1961) treats an entirely different subject, the emergence in the ante bellum period of the idea of a divided culture, it is similarly concerned with collective symbols that reconcile conflicting values. Taylor is interested, specifically, in the social conditions that produced a need for national introspection, for self-conscious pondering of the national character, and ultimately for mythmaking that led to the contrary images of Cavalier and Yankee. His thesis, if we may oversimplify an immensely complex argument, is that while Americans congratulated themselves on their newly won freedom, their prosperity, and their unlimited opportunities, they were often uncertain over the implications of severing ties with the past and destroying the forms and limits that had given meaning to life. It was not clear, for example, what class would take the place of the ruling aristocracies of Europe or what standards could be applied to evaluate political and social leaders. At least among certain strata of the population, there was a deep fear that the noblest values would be submerged in an acquisitive, materialistic civilization. The myths of the southern Cavalier and "transcendent Yankee" were thus the products of a search for ideals and stabilizing principles that would serve as a counterforce to faction and self-interest. They were "defensive fictions," in Taylor's words, which had a wide appeal in all parts of the nation. By vicariously identifying himself with the southern plantation legend, the northerner could assure himself that he belonged to something more than a purely acquisitive society. If Marx finds Webster a spokesman for the technological sublime, which harmonized nature and the machine, Taylor finds him adopting the mask of the transcendent Yankee, "a combination of Puritan and Statesman," in an attempt to reconcile ambition with high-minded patriotism. Although Taylor looks primarily to the "free fantasy" of literature for the meaning of his myths, he argues persuasively that such fiction was a projection of social needs and that literary myths had a feedback effect as statesmen played the roles of Cavalier and Yankee.

The object of this second level of cultural history is to gain an understanding of the central tensions between commitments and values that are concealed or reconciled by myths. Since a single work of art may give symbolic expression to the inner trends and strains of a culture, the problem of representativeness is not the same as for a general description of external

patterns of style. Yet there is still a danger in giving culture too much of a life of its own by reifying abstractions derived from literature. One must guard against relegating all conflict to the world of symbol and myth. It is also necessary to distinguish, as Talcott Parsons reminds us, between culture as a system of beliefs or expressive symbols that are *objects* of a social situation and culture as part of the internalized constitution of specific individuals.¹⁵ Neither of the two levels discussed thus far provides a means for studying the transmission of cultural patterns from one generation to the next or their internalization and adaptation within individual personalities.

Thus the third level of cultural history, which is still the most neglected, must search out lines of intersection between the development of culture and individual personality. The latter, of course, contains unique factors as well as a more or less integrated system of socially derived values, attitudes, and images. And because the historian must keep in touch with the actual events of the past, he must be particularly attuned to the ways idiosyncratic elements of personality have impinged upon what is regular or patterned within the culture. He must see the family not only as a vehicle of cultural continuity but as a source of innovation and adjustment to new circumstances.¹⁶ He must realize, as Kenneth Keniston has brilliantly shown in his studies of modern alienated youth, that rebels and deviants often provide the most valuable clues to the central problems of a culture.¹⁷

On this third and most specific level cultural history may assume many different forms. One is collective biography or the study of a group of individuals who are in some sense representative of a class, an occupation, an interest, or a mode of protest. In order to be significant for an understanding of the culture as a whole, such studies must gain some control over variables by precisely determining the place of the representative men within the social structure.¹⁸ Another approach, demonstrated by Erik Erikson's *Young Man Luther* (New York, 1958), a book enormously suggestive for the theory of cultural history, is to examine in detail how the personality crises of a complex individual reflect tensions within the general culture and how the individual's resolutions of conflicts within himself lead ultimately to transformations within the culture. Obviously the world has known few Luthers, and biography always runs the risk of exaggerating the historical

¹⁵ *Toward a General Theory of Action*, ed. Parsons and Shils, 3-29.

¹⁶ See, e.g., William E. Bridges, "Family Patterns and Social Values in America, 1825-1875," *American Quarterly*, XVII (Sept. 1965), 3-11.

¹⁷ Kenneth Keniston, *The Uncommitted: Alienated Youth in American Society* (New York, 1965).

¹⁸ For an example from contemporary life that also provides a careful statement of psychological theory, see M. Brewster Smith *et al.*, *Opinions and Personality* (New York, 1956).

importance of individuals or of lapsing into a superficial dualism of a man and his times. Biography may provide, nevertheless, a concreteness and sense of historical development that most studies of culture lack. And by showing how cultural tensions and contradictions may be internalized, struggled with, and resolved within actual individuals, it offers the most promising key to the synthesis of culture and history.¹⁹

Each of the three levels I have described appears to be a valid way of delimiting cultural history; yet each by itself suffers from inadequacies. Let me stress that many of the books I have used as examples actually move from one level to another and employ several methodological approaches. My main point, however, is that the three levels of conceptualization are mutually interdependent. Thus biography gains significance when related to the more general conflicts and motifs of a culture; no discussion of broad cultural styles and patterns can be complete without some awareness of the central tensions in the value system and of individual men as partial embodiments of the culture at a given moment in time. Culture is a theoretical construct that permits us to see relationships between individuals and social groups, between concrete artifacts and abstract patterns, between literature and life. And the greatest promise of cultural history lies precisely in its potential for bridging levels and filling in gaps.

If I may now turn to my personal experience, I must confess that I began my study of the American slavery controversy with little awareness of its cultural dimension. Even though I was interested in the abolitionist frame of mind, in the premises and values of reformers, my approach was basically sociological. The cultural or intellectual perspective had so long been associated with an idealistic teleology, implying that social reform was the product of a providential convergence of "streams" of secular Enlightenment and evangelical Christianity, that I was far more impressed by studies that related abolitionism to concrete social and economic dislocations. I was particularly attracted by the approaches of Avery Craven, David Donald, Stanley Elkins, and, ultimately, Tocqueville, which in varying ways related the peculiarities of American social movements to the amorphousness of the social structure, with its lack of institutional channels and moderating limits.

Increasingly, however, I discovered that the controversies over slavery involved values and associations deeply embedded in the heritage of Western culture. To put it another way, the American responses to slavery were

¹⁹ For a bold example of this approach, see Alan C. Beckman, "Hidden Themes in the Frontier Thesis: An Application of Psychoanalysis to Historiography," *Comparative Studies in Society and History*, VIII (Apr. 1966), 361-82.

framed by a whole range of symbols and associations dealing with such central concepts as liberty, authority, progress, sin, and redemption. Not only did Americans rely on precedents and arguments derived from the Bible, the ancient Greeks, or such authorities as Montesquieu; the entire controversy was colored by the basic tensions and contradictions of Western culture. Without an understanding of this cultural matrix, one could not begin to appreciate, for example, why the early abolitionists were so reluctant to repudiate the abstract legality of all human bondage. Moreover, the first individuals to take a lead in the international cause of antislavery—such men as Anthony Benezet, Granville Sharp, and Brissot de Warville—embodied within themselves the central value conflicts of their time. The cultural dimension helped to explain how problems of individual development could have a bearing on public controversy and historical events; how religious forms could become secularized in a social movement; and how an abstract issue, such as the meaning and efficacy of religion in a world of intellectual and social revolution, could be dramatized in concrete actions.

This is not to say that cultural history by itself provides a sufficient explanation of the slavery controversy. Slavery, of course, was an economic institution closely tied to various social and political structures. The dimensions of economic, social, and political history are essential for a full understanding of the subject. Yet the fact remains that it was a shift in value orientations that made possible the first organized protests against the institution. Because slavery had long been a sensitive point in the whole fabric of Western values, and had been rationalized by far-reaching religious and philosophical arguments, the very act of questioning brought deep conflicts to the surface and opened fissures in the prevailing ideologies. As groups of abolitionists began to form, they were forced, like the members of a new church, to define their relationship to established authority as well as the limits of permissible protest open to the reformer. In a word, the role of the reformer had to be shaped from the interaction of internalized values with a new social situation. Abolitionism furnished a new basis for social organization and a new means for simplifying and socializing individual moral perceptions. The concept of culture, particularly of emerging conflicts and reconciliations in shared values, helps to account for what the reformers attacked, evaded, or ignored.

It should be emphasized that cultural history can and should be combined with more conventional social, political, and economic history. At best, it brings new significance to parts of life ordinarily taken for granted and offers a perspective that permits the integration of otherwise disparate "facts." Historians have long claimed that one of the benefits of their study

is a relative freedom from the limited horizons of contemporary time, and thus from the coercions of blind and unrecognized history. Anthropologists have sought to liberate us from the screening effects of our own culture and ideology. Perhaps the ultimate goal of cultural history, as a blend of the two disciplines, is a partial transcendence of both culture and history.

1066: The "Feudal Revolution"

C. WARREN HOLLISTER*

NINE hundred years ago the Normans defeated the Anglo-Saxons at Hastings, and, ever since, historians have been pondering the meaning of this stark event and arguing over its consequences. The historical debate can be traced all the way back into the medieval chronicles, and it still rages hotly today. The present phase of the dispute began with the controversies of the later nineteenth century, centering around such figures as William Stubbs, F. W. Maitland, E. A. Freeman, and John Horace Round. H. G. Richardson and G. O. Sayles have recently demonstrated, to those not already aware of it, that Stubbs was a man of his times, with a historical vision limited by the preconceptions of his own age.¹ It is less widely understood, but equally true, that virtually all the historians of the late nineteenth century involved in the problem of the Norman Conquest were operating with a set of basic assumptions that were then unquestioned and are today unacceptable. To be specific, both Freeman and his bitter antagonist, Round, believed firmly that Anglo-Saxon England contained the seeds of democracy.

Let me reconstruct briefly a fascinating component of the Round-Freeman debate. Here is Freeman writing on Harold Godwinson:

No man ever deserved a higher or a more lasting place in national gratitude than the first man who, being neither King nor Priest, stands forth in English history as endowed with all the highest attributes of the statesman. In him, in those distant times, we can revere the great minister, the unrivalled parliamentary leader, the man who could sway councils and assemblies at his will.²

Round comments on this enthusiastic passage as follows:

We know of whom the writer was thinking, when he praised that "irresistible tongue"; he had surely before him a living model [William Gladstone], who, if not a statesman, was, no doubt, an "unrivalled parliamentary leader." Do we not recognize the portrait?³

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¹ H. G. Richardson and G. O. Sayles, *The Governance of Mediaeval England from the Conquest to Magna Carta* (Edinburgh, 1963), 1-21 *et passim*.

² E. A. Freeman, *The History of the Norman Conquest of England* (6 vols., Oxford, Eng., 1867-79), II, 352.

³ J. H. Round, *Feudal England* (2d ed., London, 1964), 304.

Round, while mocking Freeman, is yet agreeing with him. Harold was Gladstone; the Anglo-Saxons were proto-Whigs. The difference between Round and Freeman, here, lay not in the question of whether the Anglo-Saxons were democratic; both Round and Freeman assumed that they were. The difference lay in the fact that Freeman *was* a Whig while Round was a Tory aristocrat with an authoritarian's disdain for democracy.

Round continues:

But it was not an "irresistible tongue," nor "the harangue of a practised orator," of which England stood in need. Forts and soldiers, not tongues, are England's want now as then. But to the late Regius Professor, if there was one thing more hateful than "castles," more hateful even than hereditary rule, it was a standing army. When the Franco-German war had made us look to our harness, he set himself at once, with superb blindness, to sneer at what he termed "the panic," to suggest the application of democracy to the army, and to express his characteristic aversion to the thought of "an officer and a gentleman." How could such a writer teach the lesson of the Norman Conquest?⁴

Here is the old Germanist idea turned back on itself. Yes, democracy lurked in the Anglo-Saxon forests, and it destroyed the nation, as it would destroy it again if men like Gladstone had their way. Round continues to berate his dead antagonist, revealing himself in the process:

[Freeman] could not see that the system in which he gloried, a system which made the people "a co-ordinate authority" with their king, was the worst of all trainings for the hour of battle; he could not see that, like Poland, England fell, in large measure, from the want of a strong rule, and from excess of liberty. To him the voice of "a sovereign people" was "the most spirit-stirring of earthly sounds"; but it availed about as much to check the Norman Conquest as the fetish of an African savage, or the yells of Asiatic hordes. . . . Such were the bitter fruits of Old-English freedom.

And finally:

While our fathers were playing at democracy, watching the strife of rival houses, as men might now watch the contest of rival parties, the terrible Duke of the Normans was girding himself for war.⁵

Harold is Gladstone, and William is Bismarck; Harold is Newcastle, and William is the Elder Pitt. One chooses his sides according to his politics. Charles Petit-Dutaillis once said of Stubbs: "He projected into the past the image of the constitutional monarchy which he saw working under his own eyes and to which he attributed the greatness of his country." It might be said of Round that he projected into Anglo-Saxon England the image of Gladstonian liberalism which he despised. Such are the biases that underlie the turbulent historical controversy of the Norman Conquest.

⁴ *Ibid.*

⁵ *Ibid.*, 303, 305.

No one today believes in Anglo-Saxon democracy, yet throughout the twentieth century to the present, Freeman's theory of continuity across the thin red line of 1066 and Round's theory of feudal revolution have remained the two poles around which the controversy has ranged. English scholars of this century have eschewed the crude political biases of their predecessors, but they have not always succeeded in avoiding a subtler kind of bias, based on the complex antagonisms of conflicting schools of thought, personal followings, and personal loyalties. These delicate interrelationships are rooted firmly in the Round-Freeman controversy, and the biases of these two men are therefore highly relevant today. Round won the battle, though perhaps not the war. His notion of a Norman feudal revolution, asserted so boldly in the 1890's, quickly rose to the Olympian heights of received opinion, and virtually all the research done by scholars in the 1920's, 1930's, and 1940's served only to confirm and strengthen it. The most important book on the subject written in this period, Sir Frank Stenton's *First Century of English Feudalism: 1066-1166* (Oxford, Eng., 1932), incorporated Round's hypothesis in its title.

Since about mid-century, however, Round's feudal revolution hypothesis has come under attack, restrained and modest at first, but, in recent years, vigorous indeed. The past two decades or so have witnessed the emergence of a kind of "neo-Freemanism." Marjory Hollings, in 1948, urged that the Anglo-Saxon five-hide unit survived the Norman Conquest to become the basis of knights' fees in parts of the western Midlands.⁶ G. O. Sayles, in 1950, argued generally for the existence of feudalism in late Saxon England.⁷ J. O. Prestwich, in 1954, pointed to the neglected mercenary as a significant figure in Anglo-Norman warfare,⁸ thereby diminishing the military importance of the Norman feudal settlement in general and the Norman feudal knight in particular. More recently, the books of Eric John, Richardson and Sayles, and Frank Barlow, together with an accelerating flow of controversial scholarly articles in various journals,⁹ have reopened the old dispute,

⁶ Marjory Hollings, "The Survival of the Five Hide Unit in the Western Midlands," *English Historical Review*, LXIII (Oct. 1948), 453-87; see also *The Red Book of Worcester*, ed. *id.* (4 vols., London, 1934-50), IV, xx-xxxix.

⁷ G. O. Sayles, *The Medieval Foundations of England* (Philadelphia, 1950), 199 ff.

⁸ J. O. Prestwich, "War and Finance in the Anglo-Norman State," *Transactions of the Royal Historical Society*, 5th Ser., IV (1954), 19-43.

⁹ Eric John, *Land Tenure in Early England* (Leicester, 1960), 113-61; Richardson and Sayles, *Governance of Mediaeval England*, 22-135; Frank Barlow, *William I and the Norman Conquest* (London, 1965), 99 ff., and "The Effects of the Norman Conquest," in *The Norman Conquest: Its Setting and Impact*, ed. C. T. Chevallier (London, 1966), 138, and, more generally, 125-61. See also, e.g., C. Warren Hollister, "The Norman Conquest and the Genesis of English Feudalism," *American Historical Review*, LXVI (Apr. 1961), 641-63; J. C. Holt, "Feudalism Revisited," *Economic History Review*, 2d Ser., XIV (Dec. 1961), 333-40; *id.* and C. W. Hollister, "Two Comments on the Problem of Continuity in Anglo-Norman Feudalism," *ibid.*, 2d Ser., XVI (Aug. 1963), 104-18; Eric John, "English Feudalism

reviving ancient wounds and inflicting new ones, with a result that must be stimulating to some, confusing to others, and unsettling to all. At times the recently reawakened controversy has reached such levels of antagonism that one might well imagine the ghost of John Horace Round still striding fiercely along the thin red line, inspiring both his supporters and opponents with his own distinctive spirit of passionate advocacy. In 1963, Richardson and Sayles could speak of Stubbs as not simply erroneous but "dogmatic and perverse," and of Round himself as "amateurish and undisciplined," "hasty and muddled," one who "fouled the wells of truth."¹⁰ Victorian and Edwardian liberalism are gone, and, with them, the myth of Anglo-Saxon democracy. But the problems of the Norman Conquest are still capable of arousing passions. The debate over the feudal revolution remains not only interesting but explosive.

For the purpose of bringing some limited order out of the present chaos, one can divide current opinion on the question of the Conquest and military service roughly into three schools of thought: the Round school, which includes such modern supporters of the "feudal revolution" as J. C. Holt; the Neo-Freeman or "direct continuity" school represented by Richardson and Sayles, Barlow, and, perhaps, John; and a middle school to which, among other historians, I would subscribe. Advocates of the moderate position will often be found contending among themselves over various points and might well resent being made schoolmates. Their views on the problem have, nevertheless, much in common, as will be seen.

The first of these three schools, that of feudal revolution, is conveniently epitomized in Round's own words, typically lacking in self-doubt:

I am anxious to make absolutely clear the point that between the accepted view and the view which I advance, no compromise is possible. The two are radically opposed. As against the theory that the military obligation of the Anglo-Norman tenant-in-chief was determined by the assessment of his holding, whether in hidage or in value, I maintain that the extent of that obligation was not determined by his holding, but was fixed in relation to, and expressed in terms of, the *constabularia* of ten knights, the unit of the [Norman] feudal host. And I, consequently, hold that his military service was in no way derived or developed from that of the Anglo-Saxons, but was arbitrarily fixed by the king, from whom he received his fief, irrespectively both of its size and of all pre-existent arrangements.¹¹

and the Structure of Anglo-Saxon Society," *Bulletin of the John Rylands Library*, XLVI (Sept. 1963), 14-41; J. O. Prestwich, "Anglo-Norman Feudalism and the Problem of Continuity," *Past and Present*, XXVI (Nov. 1963), 39-57; John Beeler, "The Composition of Anglo-Norman Armies," *Speculum*, XL (July 1965), 398-414.

¹⁰ Richardson and Sayles, *Governance of Mediaeval England*, 30, 20.

¹¹ Round, *Feudal England*, 208.

This view, perhaps minus the *constabularia* hypothesis, still finds strong supporters.

The contrary view is aptly summarized by Richardson and Sayles, writing some seventy years later, with equal self-assurance.

The suggestion we have to make is that *servitia debita* [the baronial quotas to the king], far from having been introduced at the Conquest, were gradually established, first by the occasional settlement of disputes . . . and thereafter by reducing those of uncertain amount to something approaching a uniform standard, beginning with ecclesiastical fiefs and culminating in the general review of 1166 [the royal survey of baronial enfeoffments known as the *Cartae Baronum*]. This uniform standard was not derived from Norman practice, but appears to be associated with a widespread, traditional convention.¹²

This careful statement tends to mask the potent emotions that the two authors bring to their study. Like Freeman, they admire the Anglo-Saxons—undemocratic, to be sure, but highly civilized and richly creative. Their attitude toward Normandy can only be described as one of hostility. Normandy was “a small barbaric province” whose inhabitants “had little statecraft and little foresight . . . very little to teach and very much to learn.” Thus, “the Norman Conquest introduced no new conceptions of warfare, no new ranks of society.”¹³ The intellectual debt of Richardson and Sayles to Freeman is perfectly clear, and the two authors recognize it explicitly: “Freeman has long been under a cloud,” they write, “but it seems to us that, of all the historians during the last hundred years, he wrote the wisest words on the consequences of the Conquest.”¹⁴

Here are the two polar positions, uncompromisingly asserted, and each rooted firmly in the Round-Freeman controversy of the later nineteenth century. To many historians, including myself, an intermediate position between these two extremes seems closer to reality—that the conquering Normans contributed much and preserved much, that they introduced feudal knights’ service, but tamed it by the Anglo-Saxon tradition of royal centralization and brought it into a larger military organization that retained significant Anglo-Saxon components.

The validity of any of these three positions depends of course not on how convincing it sounds in the abstract but on how well it fits the evidence. Limited space prohibits a thorough examination of the evidence on all points at issue, but it might be useful to touch on two specific problems that are central to the general question of revolution versus continuity:

¹² Richardson and Sayles, *Governance of Mediaeval England*, 90.

¹³ *Ibid.*, 27, 61.

¹⁴ *Ibid.*, 92.

first, the alleged survival of the Anglo-Saxon "select fyrd" into the Anglo-Norman period; second, the advent of knightly quotas to the king.

Did the Anglo-Saxon five-hide territorial army—the "select fyrd"¹⁵—persist as a distinct organization in the Anglo-Norman age? This has proven to be an explosive issue indeed, for the survival of the select fyrd presents grave problems to both the "feudal revolution" school and the "direct continuity" school. To the first, an Anglo-Norman fyrd represents an Old English military institution whose existence in post-Conquest times would clearly diminish, at least to a degree, the military impact of the Norman feudal revolution. To those who follow Freeman in assuming a direct evolution of the pre-Conquest five-hide recruitment system into the post-Conquest feudal quota system, it stands as a fatal obstacle. For if the five-hide recruitment system and the arbitrary feudal quotas were employed concurrently by the Norman kings, then, obviously, the one cannot have evolved directly into the other. One must, for example, be under no illusions regarding Richardson and Sayles's rejection of the post-Conquest select fyrd.¹⁶ This rejection is not merely an appendage to their basic argument; it is absolutely essential to it.

There now exist in print studies that trace in a painstaking manner the activities of territorially recruited English troops in Norman England. One such study can be found, for example, in Professor Michael Powicke's illuminating book on military obligation in medieval England.¹⁷ Yet persuasive arguments have been set forth recently to the effect that the evidence on which these studies are built is illusory. Rather than retracing the Anglo-Norman select fyrd once again, let us look briefly at some of the evidence from an analytical rather than a chronological standpoint.

To begin with, something must be said about the general problem of evidence. There is no technical term in medieval Latin or Anglo-Saxon to describe the select fyrd. Indeed, the term "select fyrd" was only coined in 1962. For that matter, there is no contemporary technical term to describe feudal knights serving in the army in return for their tenures. These facts make it exceedingly difficult to cull from the narrative sources specific instances of the military activity of either select fyrd soldiers or feudal knights, and they make it possible for Richardson and Sayles to deny the very existence of both the Anglo-Norman feudal army and the Anglo-

¹⁵ On the Anglo-Saxon select fyrd, see C. W. Hollister, *Anglo-Saxon Military Institutions* (Oxford, Eng., 1962), 38–102.

¹⁶ Richardson and Sayles, *Governance of Mediaeval England*, 53–55.

¹⁷ M. R. Powicke, *Military Obligation in Medieval England* (Oxford, Eng., 1962), 37 ff., and, more generally, 26–47; see also C. W. Hollister, *The Military Organization of Norman England* (Oxford, Eng., 1965), 216–67.

Norman select fyrd. Most warriors of that time, they suggest, were mercenaries.

Now with all due respect to those scholars who have done valuable work in tracing the Anglo-Norman mercenary, it must be said that theirs is the happier labor. For there *are* medieval Latin terms for "mercenary," and when one encounters the words *stipendiarii* or *soliderii* in the sources, one knows that he has struck gold. Prospectors seeking traces of the feudal army are doomed to worry over such ambiguous words as *milites*, *exercitus*, *equitatio*, and *expeditio*. And the unhappy fyrd scholar (if such a term can be coined) is still less fortunate. *Fyrd* is an Anglo-Saxon word meaning simply "military force." It never appears in the Latin chronicles because it is not a Latin word. It occurs repeatedly in the Anglo-Saxon Peterborough Chronicle, but simply as a synonym for *exercitus*—"army." It is as though some future historian were trying to trace the relative importance of the US Army, Navy, Air Force, and Marine Corps in Vietnam from contemporary Vietnamese chroniclers who tended to refer only to "Those Damned Americans." Similarly, the twelfth-century monks were not interested in the details of military recruitment.

They were, however, interested in the military exploits of native Englishmen. Orderic Vital says that in 1068 the Conqueror summoned the English to his banner for the first time and led them, along with his Norman knights, on an expedition against Exeter.¹⁸ And on a number of occasions thereafter we find Englishmen—*Angli*—as a group receiving special mention in accounts of the military campaigns of the Norman kings. In 1073 and 1078 they fought on the Continent; in 1075, 1088, and 1101 they were summoned to defend the royal interest against rebellion and invasion. And evidence indicates that they were employed at other times as well.¹⁹

Who were these Englishmen? They must have been recruited somehow. Did the kings send scouts into the countryside to round them up? Were they simply responding to the ancient obligation of freemen to defend their localities in time of crisis? Perhaps on occasion they were, but this would hardly account for their use on the Exeter *expeditio* of 1068 or on the

¹⁸ Orderic Vital, *Historia Ecclesiastica*, ed. Auguste Le Prévost (5 vols., Paris, 1838–55), II, 180.

¹⁹ *Ibid.*, 176–77, 193, 254, 256, 387, III, 271–77, IV, 30–31, 174; Florence of Worcester, *Chronicon ex Chronicis*, ed. Benjamin Thorpe (2 vols., London, 1848–49), II, 2–3, 9, 10–11, 22–23, 28, 32, 48–49; William of Malmesbury, *Gesta Regum*, ed. William Stubbs (Rolls Series [hereafter cited as RS], 90, 2 vols., London, 1887–89), II, 312–13, 316, 362, 471–72; *Anglo-Saxon Chronicle*, A.D. 1067 [1068], 1068 [1069], 1071, 1073, 1075, 1079, 1087 [1088], 1091, 1093, 1101; Geoffrey Gaimar, *Lestorie des Engles*, ed. T. D. Hardy and C. T. Martin (RS, 91, 2 vols., London, 1876), I, 232; *Lanfranci Opera*, ed. J. A. Giles (2 vols., Oxford, Eng., 1844), I, 56; *Gesta Normannorum Ducum*, ed. Jean Marx (Rouen, 1914), 270, 282; Henry of Huntingdon, *Historia Anglorum*, ed. Thomas Arnold (RS, 74, London, 1879), 233.

continental campaigns. Were they mercenaries? One would not guess it from the sources, for they are described neither as *stipendiarii* nor as *solidarii*, and in 1088 King William Rufus promised that if they served him he would give them good laws, just taxes, and woods and hunting rights. He did not mention wages.²⁰

Even without additional evidence it seems more plausible than not that these Englishmen were recruited by means of the only specifically English system of recruitment that we know of: the five-hide recruitment system described in Berkshire Domesday.²¹ It has been objected that such a supposition involves an economically crushing double military obligation on the land. But this objection is hardly to the point. The chroniclers leave no doubt that there *was* a double obligation—that on occasion both Englishmen and Normans served and that they were summoned separately. The question is not how much military service the Norman kings were in fact able to wring from their subjects, but, rather, by what means they obtained it.

It can be shown that the identity between the *Angli* of the post-Conquest chronicles and the select fyrd soldiers serving from five hides is not only plausible but, to say the least, exceedingly probable. One can begin with a consideration of two passages from Domesday Book. On the estates of the bishop of Worcester four freemen held ten hides at Bishampton prior to the Conquest, rendering military service by land and sea. (The land and sea combination is characteristic of select-fyrd service but utterly alien to feudal host service.) The holders of these same ten hides in 1086 were responsible for precisely the same service.²² Again, the town of Bedford was assessed at fifty hides for service by land and sea both at the death of King Edward the Confessor and in 1086.²³ These Domesday passages admit of only two interpretations: They can be taken as demonstrating the total absence of a feudal revolution, as showing that territorial military service in 1086 was identical to that of 1066. Or to those scholars who, for other sound reasons, reject such a drastic conclusion, they can yield the hypothesis that the territorial military service of 1086 described here has nothing to do with the feudal quotas, but was a concurrent select fyrd obligation surviving from the arrangements of the Confessor's day. The passages demolish the extreme "feudal revolution" hypothesis that select fyrd service came to an abrupt end in 1066 or so and that it was then replaced by a totally new sort of service—feudal service. This is merely to say that, if the described service

²⁰ *Anglo-Saxon Chronicle*, A.D. 1087 [1088].

²¹ Domesday Book, ed. Abraham Furley and Henry Ellis (4 vols., London, 1783-1816), I, 56b.

²² *Ibid.*, 173.

²³ *Ibid.*, 209.

from Bishampton and Bedford in 1066 and 1086 was the same, then it cannot have been different.

In 1086, incidentally, the ten hides of Bishampton were divided between two vast baronial estates: those of Urse d'Abetôt and Roger de Lacy. One reviewer has suggested recently that, if the military service of Bishampton in 1086 is identified with the select fyrd, then one is driven to the absurd conclusion that Urse d'Abetôt and Roger de Lacy were to be found trudging along with the English. The notion of either of these great barons performing personal military service of any sort from these minute holdings is, of course, inconceivable.

Finally there is the well-known Hastings episode of 1094, reported in the Anglo-Saxon Chronicle and repeated, with slight variations, in Florence of Worcester and Henry of Huntingdon.²⁴ Twenty thousand English foot soldiers were summoned to Hastings purportedly for overseas service. Instead of crossing the Channel, they were fleeced by the King's agent, Ranulf Flambard, of ten shillings each and then sent home. Anyone familiar with the local support system of the select fyrd described in Berkshire Domesday will be struck by the remarkable parallelism of the two passages. Why should each of twenty thousand (or, more safely, "a multitude of") Englishmen have enough money with him that the King's agent could collect a levy of ten shillings per man? To a common foot soldier ten shillings represented, by contemporary standards, a modest fortune. The answer is provided unambiguously in the Berkshire Domesday passage attesting that in 1066 each of the five hides paid the warrior-representative four shillings for his two months of service, giving him a total of five times four shillings or twenty shillings. The parallelism between the Berkshire custom and the 1094 episode extends even to the Latin phraseology. Florence of Worcester, writing of the 1094 episode, and the Domesday scribe, writing of the Berkshire arrangements of 1066, both use the term *ad victum* in explaining why the money was paid to the soldier.²⁵

The episode of 1094 is, from the standpoint of the institutional historian, a piece of extreme good fortune, a happy bit of chance. It might not have happened; it might not have been recorded. But it did, and it was, and we are provided with a priceless glimpse of the select fyrd in 1094. Richardson and Sayles suggested that these Englishmen, like all others, were mer-

²⁴ *Anglo-Saxon Chronicle*, A.D. 1094; Florence of Worcester, *Chronicon ex Chronicis*, ed. Thorpe, II, 35; Henry of Huntingdon, *Historia Anglorum*, ed. Arnold, 217.

²⁵ Domesday Book, ed. Furley and Ellis, I, 56b; Florence of Worcester, *Chronicon ex Chronicis*, ed. Thorpe, II, 35.

cenaries,²⁶ but their suggestion will not stand close inspection. The ten shillings, quite obviously, are passing in the wrong direction—from the troops to the King, not from the King to the troops. It has even been argued that these Englishmen did not, after all, actually perform military service.²⁷ To be sure! If they had, we would have been told nothing of the ten shillings *ad victum*, but would have been provided only with another instance of unidentified *Angli* fighting overseas. Finally, it has been argued that, although these Englishmen were admittedly recruited on the Berkshire five-hide basis, they may merely have been victims of a clever bit of "fiscal antiquarianism."²⁸ This is simply to say that the one case in which a historical accident enables us actually to identify the recruitment system underlying the service of these *Angli* is, in fact, unique and that, in the other instances wherein the recruitment system underlying their service cannot be positively identified, some entirely different and unknown principle was at work. This is a possible interpretation, but an exceedingly unlikely and belabored one.

The other issue to be considered is this: were the feudal quotas of the king's tenants in chief established suddenly by William the Conqueror, or did they evolve slowly and only materialize in 1166 with the great feudal survey of Henry II, the *Cartae Baronum*? Here again there is a problem of evidence. We get no comprehensive data on feudal *servicia debita* until the *Cartae Baronum* and the scutage accounts in the Pipe Rolls of Henry II. There was no general survey of quotas during the century following the Conquest. Strictly speaking, not even the *Cartae Baronum* can be so described, for they recorded enfeoffments, not quotas. All the Pipe Rolls prior to Henry II's reign are lost save one, and that one—the roll of 31 Henry I—does not happen to record a scutage. None of these facts demonstrate that the quotas were Angevin rather than Norman; they leave the question open and send us on to more elusive evidence.

Before looking at examples of such evidence, we must make one crucial point. The five-hide select fyrd principle and the post-Conquest feudal quota principle are fundamentally different. The one was a national system governed by a standardized recruitment arrangement based on the hide; the other was a system of individual, arbitrary quotas based on private contracts and on the fee. Quite apart from the evidence, the hypothesis of an evolution from the one system to the other presents very serious, perhaps

²⁶ Richardson and Sayles, *Governance of Mediaeval England*, 54, n. 7.

²⁷ Prestwich, "Anglo-Norman Feudalism and the Problem of Continuity," 48.

²⁸ *Id.*, review of C. W. Hollister, *The Military Organization of Norman England*, *English Historical Review*, LXXXI (Jan. 1966), 107.

insurmountable, conceptual difficulties. Eric John, arguing vigorously for the direct evolution from Anglo-Saxon hidal recruitment to Anglo-Norman feudal recruitment, makes the candid statement: "At this point I had better confess that I cannot see how this was done."²⁹

Turning to the evidence, one finds once again that it is scanty, yet wherever it exists it points to the high antiquity of the 1166 quotas and, in many cases, suggests strongly that they were established by William the Conqueror. A fresh look at the evidence, in other words, suggests that Round's hypothesis concerning the Norman introduction of knights' service, narrowly conceived, still stands.

To be sure, the *Cartae Baronum* of 1166 constitute the first *comprehensive* survey of knightly enfeoffments, but there are many older records that bring the *servicia debita*, in individual cases, back to a much earlier date. There has never been any doubt that some quotas were changed slightly. Some were in dispute for generations (the bishops of Worcester argued with the monarchy for many decades as to whether they owed fifty or sixty knights). Yet the evidence is strongly in favor of a general imposition of quotas by the Conqueror.

The point of the alleged "silence of the chroniclers" has recently been raised once again, so once again the question must be examined briefly. Excluding, for the sake of argument, the thirteenth-century St. Albans chroniclers who place the imposition of quotas in 1070, there remain other far more nearly contemporary reports. The Abingdon Chronicle states that as soon as the disturbances immediately following the Conquest had abated it was noted by the King's order how many knights should be demanded from episcopal sees and abbeys for the defense of the realm. Abbot Athelhelm of Abingdon then granted estates to men who would hold them of the abbey and in each case stipulated the obligations that accompanied the tenures.³⁰ The Ely chronicler, writing midway through the twelfth century, probably well before the *Cartae Baronum*, stated that in 1088 King William Rufus summoned the knights according to the quotas that his father had imposed on the lands of the Church.³¹ Orderic, whose silence on the matter has been specifically and repeatedly alleged, declared that the Conqueror distributed lands to his knights in such a way that the realm should thereafter have sixty thousand men to answer the royal summons. The figure of sixty thousand is of course absurd—Orderic uses that particular figure repeatedly to indicate simply "a great many"—but, discounting the

²⁹ John, *Land Tenure*, 158.

³⁰ *Chronicon Monasterii de Abingdon*, ed. Joseph Stevenson (RS, 2, 2 vols., London, 1858), II, 3.

³¹ *Liber Eliensis*, ed. E. O. Blake (London, 1962), 218.

sixty thousand, we are left with the statement itself, which suggests nothing less than a revolution in military tenures instituted by William the Conqueror. Thus speaks Orderic, and it would be well, once and for all, to acquit this irrepressibly loquacious man of the charge of silence.³²

So much for the narrative sources. What of the records? First of all, the key date is not 1166 but 1156, the date of the first good Pipe Roll accounts of a scutage. The 1166 *Cartae Baronum* themselves contain comprehensive testimony to enfeoffments not only in 1166 but also in 1135 and show that enfeoffments changed only slightly between these two dates. Thus, the 1166 quotas are, by implication, carried back to 1135. And in individual cases one can carry them back much further. Fixed quotas appear in the Norman Bayeux Inquest of 1133 which almost surely attests to conditions on the Bayeux estates around 1100.

Records relating to particular fiefs sometimes enable us to carry back the 1166 quotas into the reign of the Conqueror. Anyone who is inclined to question the hypothesis that the feudal quotas were established by William the Conqueror should first look closely at the records of such estates as Evesham, Peterborough, and Abingdon. Evesham possessed estates totaling some 160 hides, many of them in Worcestershire, a county wherein the 300-hide Anglo-Saxon ship sokes have been clearly traced and in which, therefore, the related 5-hide rule surely obtained. At the rate of 1 man from every 5 hides, Evesham's 160 hides would yield a quota of 32 men to the Anglo-Saxon select fyrd. But the invaluable military summons of William the Conqueror to Abbot Aethelwig of Evesham of about 1072 alludes to a quota of only 5 men.³³ We know, from a charter of Henry I and from later Pipe Roll evidence, that Evesham's feudal quota wavered a bit, from 4½ knights to 5 knights,³⁴ but at no time did the knightly quota approach the select fyrd quota of, presumably, 32 warriors. It was 5 knights in 1166; it was 5 knights in 1072.

At Peterborough the fyrd quota is exceedingly difficult to determine because of a widespread and irregular incidence of beneficial hidation combined with the fact that many Peterborough estates were assessed in geld carucates rather than hides. In a previous article I calculated the Peterborough fyrd quota at about 70, but this is decidedly tentative.³⁵ The feudal *servicium* in Henry II's Pipe Rolls is 60, with 64 fees existing on the

³² Orderic Vital, *Historia Ecclesiastica*, ed. Le Prévost, IV, 7.

³³ *Select Charters*, ed. William Stubbs (9th ed., Oxford, Eng., 1913), 97.

³⁴ *Red Book of the Exchequer*, ed. Hubert Hall (RS, 99, 3 vols., London, 1896), I, 301-302; *Pipe Roll 18 Henry II* (Pipe Roll Society, No. 18, London, 1894), 22; *Calendar of the Charter Rolls* (6 vols., London, 1903-27), I, 257.

³⁵ See C. W. Hollister, "The Knights of Peterborough and the Anglo-Norman Fyrd," *English Historical Review*, LXXVII (July 1962), 417-36.

estates in both 1166 and 1135.³⁶ Of these 64 fees, 62 are described by Walter of Whittlesey and John of Peterborough as having been created by Abbot Turolde in about 1070.³⁷ The feudal quota of 60 is very high considering the size of the Peterborough estates, and, in consequence, the individual Peterborough knights' fees are singularly small.³⁸ It seems most likely that the 60-knight *servicium debitum* was a punitive quota imposed by the Conqueror in reply to the support given by Peterborough tenants to Hereward's rebellion. Indeed, Hereward himself appears to have been a Peterborough tenant.³⁹

Abingdon held some 624½ hides in 1066, chiefly in Berkshire—where the 5-hide rule is unquestioned—with some land in Oxfordshire and a little in Gloucestershire. The assessment of 624½ hides in 1066 was reduced by 1086 to 425¼. Dividing these figures by five, one gets 5-hide fyrd quotas of about 125 men in 1066 dropping to about 85 men in 1086. But a knight list of William the Conqueror's time in the Abingdon Chronicle discloses only 31 fees.⁴⁰ The same chronicle reports that nearly all the Abingdon knights were required to join King William's Welsh campaign of 1081, and one might reasonably conclude that on this occasion Abingdon must have sent some 30 knights or so to the royal summons.⁴¹ An almost identical number—33 fees—is disclosed in the *Carta* of 1166, and Henry II's Pipe Rolls attest to a feudal quota of 30 knights.⁴² Thus once again the enfeoffments of the Conqueror's time, utterly unrelated to the fyrd quota, are tightly correlated to the feudal quota as it emerges in Henry II's Pipe Rolls.

Robert S. Hoyt, incidentally, has observed in an unpublished paper that at Abingdon the double burden on the land of 5-hide quota and feudal quota is more than balanced by the Conqueror's reduction in hidage. In 1066, at 1 man per 5 hides, Abingdon owed 125 men to the select fyrd. In 1086, with the reduced hidage assessment, the same Abingdon estates owed 85 men to the select fyrd plus 30 knights to the feudal host, or a total of 115 men to both forces. In short, the single obligation in 1066 required 125 men; the double obligation of 1086 required 115 men. It must

³⁶ *Red Book of the Exchequer*, ed. Hall, I, 329; cf. *Chronicon Petroburgense*, ed. Thomas Stapleton (London, 1849), 168–75, where, in a document dating between 1113 and 1120, 64 Peterborough fees are reported.

³⁷ Walter of Whittlesey, in *Chronicle of Hugh Candidus*, ed. W. T. Mellows (London, 1949), 84, n. 4; John of Peterborough, *ibid.*, 85 n.

³⁸ In the Peterborough *Descriptio Militum* of about 1113–1120, fees of less than one hide are reported. (*Chronicon Petroburgense*, ed. Stapleton, 169–75.)

³⁹ Cf. *ibid.*, 175 (Ansford), with Domesday Book, ed. Furley and Ellis, I, 346, 376b.

⁴⁰ *Chronicon Abingdon*, ed. Stevenson, II, 4–6; cf. Domesday Book, ed. Furley and Ellis, I, 566–96.

⁴¹ *Chronicon Abingdon*, ed. Stevenson, II, 10.

⁴² *Red Book of the Exchequer*, ed. Hall, I, 305–306; *Pipe Roll 14 Henry II*, 203; *Pipe Roll 18 Henry II*, 15; see also D. C. Douglas, "Some Early Surveys from the Abbey of Abingdon," *English Historical Review*, XLIV (Oct. 1929), 613–25.

be added that other fiefs do not yield such tidy results, but it remains a possibility that there may be some significant connection between the Conqueror's uncharacteristically generous policy of widespread beneficial hidation and the establishment of a twofold military obligation on the land.

The quotas of Evesham, Peterborough, and Abingdon—five knights, sixty, and thirty—are all divisible by five. In this respect they are characteristic of the feudal quotas in general. The *Cartae Baronum* and Henry II's Pipe Rolls disclose a strikingly symmetrical system of round quotas. There are some exceptions to this rule, but the tendency is strong and unmistakable. Quotas of five knights, ten, twenty, forty, sixty, and so on, occur repeatedly. Round associated these figures with his hypothetical ten-knight constabularies. He was doubtless mistaken, yet clearly they suggest the work of a single authoritative assessor. If one can be forgiven for applying a hoary theological argument to this mundane problem, it may be said that such remarkable symmetry as one finds in the English feudal quotas cannot have been the accidental result of individual bargains hammered out over the decades; nor can it have arisen from the older five-hide recruitment system. For if one calculates the hides of the tenants in chief and divides by five, the resulting quotas are not round but bewilderingly miscellaneous. A single assessor was evidently responsible for this coherent structure of feudal quotas, and, in the light of all the evidence, one can confidently identify the assessor as being William the Conqueror.

The Round hypothesis, that the English feudal quotas were established by the Conqueror, has traditionally carried the corollary that William brought the feudal quota system into England from Normandy. Round assumed this without serious investigation, and his assumption was apparently confirmed early in the present century by Charles Homer Haskins.⁴³ Although Haskins' sources dated for the most part from the post-Conquest period, and largely from the twelfth century, he nevertheless argued persuasively that a coherent and encompassing system of feudal quotas existed in pre-Conquest Normandy and, indeed, served as William's model. Such a conclusion seemed unarguable, for if the Anglo-Norman *servicia debita* were introduced by the Conqueror, and if they had no Anglo-Saxon roots, where else might they have come from besides Normandy?

More recently, however, Haskins' conclusion has been effectively qualified in the writings of David Douglas, Lucien Musset, and Joseph R. Strayer.⁴⁴ As Strayer has pointed out, fixed quotas are conspicuously absent

⁴³ C. H. Haskins, *Norman Institutions* (Cambridge, Mass., 1918), vii, 8 ff., *et passim*.

⁴⁴ David Douglas, "The Rise of Normandy," in *Proceedings of the British Academy*, 1947

in *enfeoffment* charters issued by dukes of Normandy prior to the Conquest.⁴⁵ And although such quotas may have been developing, at least on Norman ecclesiastical estates, on the eve of 1066, they appear to have been not only far lower but also much less comprehensively established than those imposed by the Conqueror on the estates of post-Conquest England. In short, the feudal quota system of William the Conqueror's England was not a purely Norman importation. Rather, it was a bold extrapolation of limited and undeveloped Norman precedents, greatly expanded and systematized by the Conqueror as he exploited the vast opportunities afforded by his power over a conquered land. It is possible, therefore, to speak not only of the Norman impact on the development of English feudalism but also of the English impact on the development of Norman feudalism. The key word is not "importation" but "interaction." "It may be," writes Strayer, "that Normandy was made to conform to the English model, rather than the reverse."⁴⁶ Or, as Douglas has put it, "If English feudalism was essentially Norman, so also was Norman feudalism by the end of the eleventh century, in some sense, English."⁴⁷

Such, then, are the implications of some of the evidence regarding certain crucial issues of the Norman Conquest. There are many other issues, and the diverse interpretations of various historians are apt to convey the impression of confusion and chaos. But, despite this appearance, one can detect a broad consensus developing in the independent and often concurrent investigations of many modern scholars of the Conquest. Agreement is assuredly not complete; far from it! Yet as one examines the works of such men as Michael Powicke, J. O. Prestwich, R. C. Smail, John Beeler, David Douglas (in his *William the Conqueror*), and others, he can see, beneath the surface controversy, a general consensus with varying emphases. All agree that the *servicia debita* were imposed by the Conqueror, that they represent an institutional break from the Anglo-Saxon past, that there were, nevertheless, important elements of institutional continuity between the military organizations of Saxon and Norman England, and that the first century of English feudalism was not so feudal as has sometimes been thought. Sir Frank Stenton himself, of course, was by no

(London, 1948), 101-30, and *William the Conqueror* (London, 1964), 281-84 *et passim*; Lucien Musset, "Aux origines de la féodalité normande," *Revue historique de droit français et étranger*, 4th Ser., XXIX (1951), 150; Joseph R. Strayer, review of *Recueil des actes des ducs de Normandie*, 911-1066, ed. Marie Fauroux, in *Speculum*, XXXVII (Oct. 1962), 609-10.

⁴⁵ *Recueil des actes des ducs de Normandie*, 911-1066, ed. Marie Fauroux (Caen, 1961), Nos. 80, 140.

⁴⁶ Strayer, review of *Recueil des actes*, ed. Fauroux, 610.

⁴⁷ Douglas, *William the Conqueror*, 283.

means insensitive to the continuity of institutions across the line of 1066. Smail has written:

Even from the late-eleventh century, when English feudal institutions were still in process of formation, the Conqueror and his sons after him relied on non-feudal sources of recruitment. It is doubtful whether the military needs of the English kings could ever have been met from feudal sources alone.⁴⁸

Michael Powicke, in his fine book on military obligation in medieval England, stresses both the importance and the limitations of William I's new feudal army and gives due attention to the Anglo-Norman mercenaries and the English.⁴⁹ Beeler, in his articles and in his illuminating new book on English medieval warfare, puts slightly greater stress on the service of the knightly feudal contingents, but presents the same heterogeneous picture of the Anglo-Norman army and makes the same distinction between *fyrð* and feudal host.⁵⁰ Prestwich, although hostile to the renewed emphasis on institutional continuity, has himself recently suggested an important and previously neglected example of it in the military households of the Saxon and Norman kings, which constituted the cores of pre- and post-Conquest English armies.⁵¹ Douglas has written recently: "The successful imposition of tenure by service upon his magnates in respect of their English lands must be regarded as one of the most notable of the Conqueror's achievements." But he also states that "the Norman impact upon England was to be drastically modified by English tradition under the direction of the Norman king."⁵² And Douglas emphasizes the Conqueror's use of the five-hide *fyrð* and mercenaries. These statements epitomize the fundamental position that is coming more and more to be accepted.

It is good to have controversy, but it is good, also, to find that the majority of investigators are not looking at the problem in totally diverse ways, to find some reason for believing that, as our knowledge grows, there is developing the tendency for dispassionate scholars to agree on certain fundamental points. And at the moment there is reason for optimism regarding the problem of the Norman Conquest.

⁴⁸ R. C. Smail, "The Art of War," in *Medieval England*, ed. A. L. Poole (2 vols., Oxford, Eng., 1958), I, 137.

⁴⁹ Powicke, *Military Obligation in Medieval England*, 1-47 *passim*.

⁵⁰ John Beeler, "Composition of Anglo-Norman Armies," *passim*, and *Warfare in England, 1066-1189* (Ithaca, N. Y., 1966).

⁵¹ Prestwich, "Anglo-Norman Feudalism and the Problem of Continuity," 50-52.

⁵² Douglas, *William the Conqueror*, 273, 265.

The Aims of Drake's Expedition of 1577-1580

K. R. ANDREWS*

FOR three centuries and more it was generally assumed that Drake's voyage round the world was a deliberate act of reprisal against Spain, sponsored principally, though more or less covertly, by the Queen, and that in plundering Spanish shipping and settlements on the Pacific Coast of America he was implementing the main intention of his promoters. J. S. Corbett's authoritative biography did not radically revise this traditional account, but represented the venture as primarily a "project for an attack by sea upon Panama."¹ In 1914, however, Zelia Nuttall, in her edition of Spanish materials relating to the voyage, suggested that the chief aim was to explore the Pacific Coast of America with a view to colonization, plunder being a secondary consideration.² Her theory in particular and the prevailing views in general came under heavy fire in 1926, when the American scholar H. R. Wagner produced an elaborate analysis to demonstrate that "the expedition was essentially one for trading purposes, directed to the Moluccas and perhaps China" and that "the diversion of the expedition en route into a raid on Spanish commerce on the west coast of America was accomplished by Drake himself without the consent of the representatives of the adventurers."³ The critical sophistication of Wagner's argument was impressive enough to discredit rival interpretations, but his own hypothesis did not long remain intact. His declaration that "it is hardly likely that any other document of real value will be discovered in the future" was followed within four years by the publication of two newly found pieces of evidence that provided the keys of a fresh interpretation by E. G. R. Taylor.⁴ She

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¹J. S. Corbett, *Drake and the Tudor Navy* (2 vols., London, 1898), I, 212. Corbett also wrote that the venture was "in a way a complement to [Martin] Frobisher's," but meant no more by this than that Drake proposed to enter the Pacific by the southern strait and Frobisher by the northern. Elsewhere he credited Drake with "projects of colonial and commercial expansion" in northwest America and the East Indies and maintained that he made some attempt to find the Strait of Anian, but these appear as by-products of the main purpose.

²Zelia Nuttall, *New Light on Drake* (London, 1914). The main emphasis is on "Drake's dream" of an "Anglo-Saxon" empire in North America, though the search for the Strait of Anian is also presented as an important aim.

³H. R. Wagner, *Sir Francis Drake's Voyage around the World* (San Francisco, 1926), 26-27.

⁴The summary in E. G. R. Taylor, *Tudor Geography, 1485-1583* (London, 1930), 110-19,

concluded that Drake's instructions were to explore the Pacific Coast of the supposed southern continent, *Terra Australis*, and to go on to the Spice Islands; after the Moluccas, or perhaps as an alternative to them, he was to seek the Strait of Anian, the supposed Pacific approach to the Northwest Passage. In the event Drake, with the connivance of the Queen, converted this peaceful expedition of discovery and trade into a raid for plunder, though he did make some attempt to find the northern strait and did eventually reach the Moluccas. This explanation, partly subsuming and partly contradicting Wagner's, was and is still generally accepted, and the purpose of this article is to re-examine the arguments on which it is based and, without introducing any fresh evidence, to offer an alternative interpretation.

The claim that the true objects of the voyage were peaceful depends partly upon Wagner's explanation (followed by Taylor) of Drake's quarrel with Thomas Doughty. Doughty and John Winter, it is argued, disapproved of Drake's adoption of a course of plunder, and Doughty's opposition led to his execution. The evidence for this consists of a statement made by Winter after his return concerning the capture of a Portuguese vessel at the Cape Verde Islands. He protested that this action

was utterly contrary to my good will whiche I could not lette nor gaynsaye, for that I had noe aucthority ther, but suche as pleased the sayde Drake, to give and take awaye from me att his will and pleasure, and being in greate feare of my liffe yf I shoulde have contraryed him or gone aboute to practise to withestande him in any parte of his doeinges, he woulde have punished me by deathe, for that his wordes and threateninges many tymes tended there unto by open spetches as by example of a gentellman whome he executed afterwarde.⁵

Winter's statement was in fact a deposition in the Admiralty Court made in connection with a Portuguese claim for restitution, and his main concern was to exculpate himself. The records of the court show that explanations offered in such circumstances were more often false than true. Even if true, however, the statement neither says nor implies that Doughty objected to or even privately disapproved of the seizure. The narratives of John Cooke⁶ and Francis Fletcher⁷ show that Doughty was placed in charge of the prize and became involved in a squabble with some of Drake's men over

is based on her three articles: "Master John Dee, Drake and the Straits of Anian," *Mariner's Mirror*, XV (Apr. 1929), 125-30; "More Light on Drake," *ibid.*, XVI (Apr. 1930), 134-51; and "The Missing Draft Project of Drake's Voyage of 1577-80," *Geographical Journal*, LXXV (Jan. 1930), 44-47.

⁵ Nuttall, *New Light on Drake*, 386-91.

⁶ *The World Encompassed by Sir Francis Drake*, ed. W. S. W. Vaux (London, 1854).

⁷ *The World Encompassed and Analogous Contemporary Documents*, ed. N. M. Penzer (London, 1926).

the prize goods. Later, at his trial, no suggestion was made that he favored a peaceful voyage. On the contrary, charged that he had said "that the queen's majesty and council would be corrupted," he admitted he might have said that "if we brought home gold we should be the better welcome."⁸ As for Winter's attitude, a better indication is given in his other statement concerning the voyage, one devoted largely to explaining to Burghley how and why he had parted company with Drake and returned home. He implied approval of the capture in the following terms: "Here Francis Drake, in the Pelican, took a very plentiful prize of wines and bread, with other necessities, which with good order would have done good to his company, but the discommodities that it bred through disorder, I leave for brevity."⁹ The reference to "discommodities" and "disorder" incidentally provides a clue to a more convincing explanation of the Doughty-Drake quarrel.

Doughty's trial raises another question relating to the purpose of the voyage. In the course of the proceedings Doughty admitted having given Burghley a "plot" (that is, a plan) of the voyage, whereupon Drake accused him of treachery, "for her Maiestie gave me speciall commaundement that of all men my lord Tresorar shuld not knowe it."¹⁰ If the voyage were to be a peaceful one, why should not Burghley have been informed of its intended course? Wagner's suggestion—that the promoters were afraid of the opposition of the Muscovy Company and that Burghley might be expected to take its side—is little short of absurd. The monopoly of the company extended only to northerly explorations, and it had only recently been compelled by official pressure to permit Frobisher's Northwest Passage venture, which actually did infringe its rights. The notion that Elizabeth for this reason concealed a perfectly innocuous enterprise in which she had a strong personal interest cannot be taken seriously. Nor does Taylor offer any solution. On the broader question of why the preparations should have been carried out, as they were, with extreme secrecy, under the cloak of a voyage to Alexandria, she argues that the Iberian countries might well have raised objections to a project for discovery in the South Pacific, a region hitherto monopolized by them. If, however, the promoters were genuinely anxious (as she maintains they were) to avoid trespassing on Iberian territory, the thoroughness and efficiency of the security arrangements, which were unsurpassed in the annals of Elizabethan maritime enterprise, appear disproportionate to the requirements of the situation.

⁸ *World Encompassed*, ed. Vaux, 203.

⁹ Taylor, "More Light on Drake," 148.

¹⁰ *World Encompassed*, ed. Vaux, 204.

More particularly, this hypothesis fails to explain Drake's violent reaction to the revelation that Burghley was informed of the plans.

Thus the Doughty affair provides no support for the thesis that the true purposes of the voyage were peaceful. On the contrary, it suggests that neither Winter nor Doughty opposed a course of plunder and that the intentions of the promoters had sufficiently dangerous implications to warrant their concealment from Burghley.

The establishment of trade with the Moluccas was, in Wagner's view, the prime object of the voyage, and in Taylor's interpretation this objective is also emphasized as a major one, second only to the discovery of *Terra Australis*. It should be noted first that although this was one of the most richly documented Elizabethan voyages, no contemporary statement to either of these effects has ever been found. Doughty, who had every reason to raise such a point in his defense, failed to do so; Cooke, a bitter critic of Drake's conduct, does not mention the Moluccas once in his narrative. The only statement in existence directly concerning the original aims of the voyage (the draft plan, discussed below) omits all reference to the Moluccas. The case for the Moluccas as an objective depends entirely on interpretations of events during the voyage.

A piece of evidence that Wagner claims to afford "the strongest kind of proof that the original intention was to go to the Moluccas" is the account in the "Famous Voyage" of Drake's intentions after the capture of the *Cacafuego*: Drake

began to consider and to consult of the best way for his Countrey. He thought it not good to returne by the Streights, for two speciall causes: the one, lest the Spaniards should there waite, and attend for him in great number and strength, whose hands, hee being left but one ship, could not possibly escape. The other cause was the dangerous situation at the mouth of the streights in the South sea, where continuall stormes reigning and blustering, as he found by experience, besides the shoalds and sands upon the coast, he thought it not a good course to adventure that way: he resolved therefore to avoyde these hazards, to goe forward to the Islandes of the Malucos, and therence to saile the course of the Portugals by the Cape of Buena Esperenza.¹¹

If this passage indicates anything about the original intention, it is that it was to return via the Strait of Magellan. It categorically states that the Moluccas course was adopted only as an alternative way home. This does not prove that the promoters intended him to return by the Strait of Magellan, but it is a fact that the only document we have concerning the promoters' intentions—the draft plan—instructed him to do precisely that.

¹¹ Richard Hakluyt, *The Principal Navigations, Voyages, Traffiques & Discoveries of the English Nation* (12 vols., Glasgow, 1903-1905), XI, 118.

To the master of the *Cacafuego* Drake gave a letter of safe-conduct addressed to Winter, alleged to contain words to this effect: "What we determined about the return to our country will be carried out if God so wills." It is argued from this that "as Drake then intended to return by way of the Moluccas, this message affords proof that this was the route to which he referred, and was the one agreed upon between him and Winter and [John] Thomas before the vessels separated, in fact before the expedition left England."¹² The inference is by no means admissible. In the first place, the authenticity of the passage cited is open to question. This version is Nuttall's retranslation of a Spanish translation made for the benefit of his interrogators by John Butler, John Oxenham's fellow prisoner.¹³ Richard Hakluyt's retranslation of the Spanish translation given in Nuño da Silva's report reads: "and what composition or agreement we have made, at my returne into England I will by Gods helpe perfourme," which suggests that whatever was agreed did not concern the choice of the return route.¹⁴ Secondly, Drake's intentions at this stage are not known: Taylor's interpretation and much of the evidence suggest that he was thinking of returning by the Strait of Anian. Supposing that Nuttall's version is correct and that it refers to the choice of route, it remains impossible to determine which route was meant, and the agreement might have been that Drake and Winter should take different routes. Thirdly, the passage implies nothing about the plans laid in England. If Drake and Winter between them made some decision about their return route or routes, that decision was not necessarily simply to carry out their original instructions; indeed, a reference to their own decision might be taken to imply that the captains altered the original plan by that decision. Finally, the passage explicitly refers not to the objectives of the voyage but to the problem of how to get back to England.

Wagner's strongest argument is based on a passage from Fletcher's narrative:

wee following the directions of the comon Mapps of the Spanyards were utterly deceived for of a Malitious Purpose they had set forth the Mapp false that they might deceave strangers if anny gave the attempt to travaile that way that they might perish by the Running ofe to the Sea, rather then Touch with anny part of the land of America. for where the land trendeth & lyeth to the north east or rather to the eastward they in their Mapps at the least 12 degrees have layed it out to the Norwest by reason wherof wee lost the wind *which* would have caried us right in our way & held the right course to the unfortunate Ilands wherewith Magilanus so unhappily fell in the first Voyage about the world

¹² Wagner, *Sir Francis Drake's Voyage*, 25.

¹³ Nuttall, *New Light on Drake*, 16.

¹⁴ Hakluyt, *Principal Navigations*, XI, 147.

which way wee had helden if the wind had served but being boath right against us & so violent that it was intollerable wee were Enforced back againe.¹⁵

This is merely a statement of what Fletcher believed to be Drake's intention on leaving the strait. Fletcher was almost certainly mistaken. The meaning of the passage is not completely clear, but it apparently contradicts several other sources, which represent the immediate objective at this stage as "Peru," or more precisely a rendezvous thirty degrees south on the coast of Chile. Doughty, for example, when condemned to die, begged Drake to "cary me with you to the Perwe and there set me a shore."¹⁶ Winter excused his return from the strait chiefly on the grounds that he despaired of a wind "for to go to the Peru,"¹⁷ a point underlined in Edward Cliffe's narrative.¹⁸ *The World Encompassed* and Nuño da Silva's relation both expressly refer to the arrangement, before the ships separated, of a rendezvous in thirty degrees south.¹⁹ On emerging from the strait, the three vessels held their course northwest (the supposed direction of the coast) for three days, covering a distance of some seventy leagues and clearly making for "Peru."²⁰ Fletcher's statement may be reconciled with the others by assuming that he carelessly omitted to refer to "Peru" and really meant that the intention was to sail west from "Peru" at a later stage. There is no reason to believe that Fletcher was in Drake's confidence; the two probably distrusted each other and certainly fell out later in the voyage. The statement may, on the other hand, reflect a failure of memory, for Fletcher's reference elsewhere to Thomas Cavendish's circumnavigation shows that his manuscript was not finished until at least ten years after the event. Finally it must be noted that the statement, whether true or untrue, refers to Drake's intention on leaving the strait and consequently proves nothing about the original purpose of the venture.

The thesis that Drake and Winter before parting company agreed on a course that should take in the Moluccas, however, finds stronger support in a document unavailable to Wagner: Winter's report on his voyage. This describes how, after entering the South Sea, he was forced back into the strait, where he "spent 22 days, and looked still for Mr Drake, and for a change of wind." Then, "being out of hope almost of a wind, and of his safety, except he should be to the leewards of us, persuaded with my Master and some of my company for the Moluccas." A week later, "calling my whole company together, I made my determination generally known,

¹⁵ *World Encompassed*, ed. Penzer, 132.

¹⁶ *World Encompassed*, ed. Vaux, 207.

¹⁷ Taylor, "More Light on Drake," 151.

¹⁸ *World Encompassed*, ed. Vaux, 281.

¹⁹ *Ibid.*, 83; Nuttall, *New Light on Drake*, 262.

²⁰ *World Encompassed*, ed. Vaux, 259; Nuttall, *New Light on Drake*, 280.

which was for the east parts of the world, using what persuasion I could. And protested unto them upon the Bible that Mr Drake told me that he would go thither when I was last aboard of him." The master and crew refused to sail such a course and so,

hopeless of my determined voyage, as also of the finding of Mr Drake, and despairing utterly of the favourableness of the wind for to go to the Peru (which was continually betwixt the W. S. W. and the N. from the 20th of June to the 20th of August, and continued there betwixt the N. W. and the W. from the 20th of August till the 11th of November, and changed not till we came into 40 degrees), and thus, as I have said before, standing out of hope of wind, the 11th of November I bare room with my country whilst I had the wherewithal.

Finally Winter reveals the point of the document: "I stand nothing doubtful that this my return will be to the liking of Her Majesty, beneficial to my country, and no discredit to my name. Which if it should prove to the contrary, it should not be pleasant to me to live."²¹

It should be unnecessary to stress that a statement manifestly written to justify a course of action that might be interpreted as desertion cannot be taken at its face value. The assertion that it "disposes, once and for all, of the slanderous accusation, made by Edward Cliffe and published by Hakluyt, that it was 'against the mariners' minds' that Winter returned through the Straits"²² is unacceptable. Cliffe's account was a plain, factual report with no *parti pris*, and on this point his version is independently corroborated by Cooke. There is reason, therefore, to doubt the strength of Winter's "determination" to go to the Moluccas. On the other hand, since the story was designed to convince Burghley, the reference to the Moluccas could hardly have been totally implausible, and we know from Spanish sources that Drake and his men told more than one of their prisoners that their consorts, having failed to rejoin, were presumed to have gone to the Moluccas. The fair conclusion is that there had been some talk of visiting the Moluccas, and perhaps some plan arranged by Drake and Winter before they lost company.

But the report provides no grounds for the argument that the Moluccas were the original objective. It says clearly that Winter determined to make for the Moluccas because he could not, by reason of adverse winds, set course for "Peru." As for Drake, he had ordered a rendezvous in "Peru," and he kept it. If he mentioned the Moluccas to Winter, he cannot have talked of sailing there direct. In effect, there is nothing here to suggest an intended westerly course to the Moluccas, either directly or via the coast

²¹ Taylor, "More Light on Drake," 147-51.

²² *Ibid.*, 138.

of *Terra Australis*. On the contrary, according to Winter's account of the winds, it would have been even harder to make headway westward than northward toward "Peru." It must be concluded that in referring to a Moluccas course Winter meant an easterly one, and there happens to be independent support for this view. One version of the master of the *Cacafuego's* deposition says that Drake's men judged that their consorts "were gone toward the Moluccas, by the island of Madagascar, or St. Lawrence."²³ The reference to Madagascar (alias St. Lawrence) is probably an interpolation by the sixteenth-century English translator, but taken in conjunction with the evidence of the winds it seems to offer the only reasonable interpretation. The course Winter contemplated involved, not the fulfillment of the original purpose, but the abandonment of what all writers agree was the essence of the project: penetration to the Pacific by way of the Strait of Magellan. Had the Moluccas been the main objective, or at least an essential part of the program, Winter would surely have drawn Burghley's attention to his own fidelity to the promoters' instructions, pointing out in self-defense that Drake had deserted him, not he Drake. He would surely have reminded his master and crew of their obligations, instead of appealing, rather feebly, to what Drake had said at their last meeting and proposing the Moluccas as a second-best alternative to "Peru."

To summarize the evidence under this heading, the immediate objective from before the death of Doughty until after the passage of the strait was undoubtedly the coast of Chile. The safe-conduct indicates that Drake and Winter at some stage agreed on a plan for returning home, and Winter's report suggests that they did so at their last meeting, which presumably took place during the clear month that the ships remained in company after leaving the strait. In view of the evidence that Drake later contemplated the northern passage as a way home, we cannot say on the basis of the prisoners' statements and Winter's report (all more or less unreliable) that any definite decision was made to return via the Moluccas, though it seems likely that the Moluccas route was discussed. The essential clue to the place of the Moluccas in the project is that no mention of them whatever occurred before the fleet entered the Pacific. Then, of course, there was good reason to mention them, for the appalling storms of the next month and the loss of the *Marigold* must have convinced Drake that to return via the Strait of Magellan would be to court disaster. At this stage he clearly intended coasting northward toward thirty degrees, and knowing, as he probably did, that a westward course in that latitude was feasible, he presumably

²³ British Museum [hereafter cited as BM], Lansdowne MS. 122, fols. 22-28.

recognized this as a preferable route home. Thus the various references to the Moluccas intention in no way clarify the original aims of the voyage.

Apart from the weakness of the evidence on which it is based, Wagner's theory (and Taylor's so far as it concerns the Moluccas) is open to a number of objections. If the voyage were purely peaceful in intent, is it not strange that Drake, a man already noted for his piracies, should have been chosen for command? In a venture of trade reconnaissance of such importance it was usual to carry the Queen's letters to foreign potentates recommending her envoy and requesting peace and trade, but Drake apparently carried no such missives and had no written communication with the Sultan of Ternate, the rajahs of Java, or other rulers. He does not appear to have been empowered to negotiate trade treaties. On arriving in the East Indies he seems to have lacked any preliminary knowledge of the situation there. He spent only five days at Ternate and refused invitations to go ashore. There is no mention of trade goods, either in the list of equipment in the draft plan (see below) or elsewhere. An allowance of fifty pounds was made for presents for "the lords of the countries," but this was hardly sufficient for gifts worthy of the Moluccas sultans. In a commercial expedition one might expect to find a number of merchant investors and supercargoes, but the only list of promoters contains no merchants, while only one member of the expedition can be identified as a merchant. No claims upon the return cargo were made by merchants, so far as is known. Finally, if the Moluccas were the prime objective, why should the fleet have taken Magellan's rather than the Portuguese route? In terms of the geographical ideas prevalent in 1577 there was little to choose between them in respect of distance, while the regularly used Cape route must have looked less difficult nautically. When Edward Fenton was sent out to follow up Drake's contact with the Spice Islands, he was ordered to take the Cape route and forbidden to pass the Strait of Magellan. The only other East Indies voyages of a clearly commercial character during the reign—those of James Lancaster in 1591 and 1600—went via the Cape. Only the would-be plunderers of Peru—Cavendish, John Chidley, and Richard Hawkins—took the notoriously dangerous way of Magellan.

Taylor's answer to the last-mentioned problem is that the primary interest of the adventurers lay in exploration and trade reconnaissance in the South Pacific, especially the continent of *Terra Australis*, the Moluccas being the obvious final destination and complementary objective of a voyage westward from the strait, coasting that continent. There is persuasive circumstantial evidence of a heightening of English interest in the South

Pacific in the 1570's. Quickening news of the achievements of Miguel López de Legaspi, Andrés de Urdaneta, Alvaro de Mendaña, and Pedro Sarmiento reached England early in the decade. It is argued, therefore, that when "divers gentlemen of the west part," led by Richard Grenville and William Hawkins, petitioned in 1574 for leave to go in search of lands "southward beyond the equinoctial" not already in the possession of any Christian prince, they had *Terra Australis* in mind.²⁴ The intention was clearly to pass the Strait of Magellan, for a draft of letters patent to Grenville and his associates mentions trade with Cathay,²⁵ and John Oxenham told his Spanish interrogators that Grenville wanted to found a colony on the Plate River and then "pass the Strait and establish settlements wherever a good country for such could be found."²⁶ There is nevertheless, no mention of *Terra Australis* in the petition or elsewhere in the Grenville material. Oxenham said that the Queen disapproved of the scheme "because she had learnt that beyond the Strait of Magellan there were settlements made by the Spaniards, who might do them harm," and one of his fellow prisoners deposed that the Queen demanded a large sum as security that Grenville "would not touch lands belonging to King Philip." The only Spanish settlements in the South Pacific were on the mainland of South America, and so it appears that the Queen (or was it really Burghley?) suspected Grenville of designs upon the unoccupied southern part of that coast, which bordered upon Spanish territory, as well she might in view of the piratical reputations of Grenville and Hawkins.

Taylor has shown that John Dee was interested in Marco Polo's land of Locach or Beach, associated by some with Solomon's land of Ophir and located south of the Oriental Guinea, not far from Mendaña's discovery, on the continent of *Terra Australis*. Dee's friends at court included Christopher Hatton and the Earl of Leicester, both promoters of Drake's venture, not to mention Elizabeth herself. It is possible that he knew of their project and offered his opinions and advice. But the most certain fact about the plan of the voyage is that the Pacific was to be reached by way of the Strait of Magellan, a course that Dee never advocated, his interest being focused upon the northern passages, especially the northeastern. It is doubtful whether Dee had any influence upon the promoters' plans.²⁷

²⁴ Public Record Office, State Papers Domestic, Elizabeth, XCV, 64.

²⁵ *Ibid.*, CCXXXV, 1.

²⁶ Nuttall, *New Light on Drake*, 5-12.

²⁷ The items in his private diary cited in Taylor, *Tudor Geography*, 117, add up to very little. The visit from Leicester, Edward Dyer, and Sir Philip Sidney occurred ten months before Drake's departure and only four months before Frobisher's. The Earl of Bedford, although Drake's godfather, is not known to have had any connection with his maritime career. The other meetings occurred after Drake had sailed.

By far the most important item of evidence relating to the original purpose of the voyage is the draft plan discovered by Taylor in the Cottonian Manuscripts. As reconstructed by her, the crucial passage concerning the objectives reads:

*. . . shall enter the Strait of Magellanas lying in 52 degrees of the pole, and having passed therefrom into the South Sea then he is to sail so far to the northwards as xxx degrees seeking along the said coast aforenamed like as of the other to find out places meet to have traffic for the venting of commodities of these her Majesty's realms. Whereas at present they are not under the obedience of any christian prince, so there is great hope of gold, silver, spices, drugs, cochineal, and divers other special commodities, such as may enrich her Highness' dominions, and also put shipping a-work greatly. And having gotten up as afore said in the xxx degrees in the South Sea (if it shall be thought meet by the afore named Francis Drake to proceed so far), then he is to return by the same way homewards as he went out. Which voyaging by God's favour is to be performed in xiii months, all though he should spend v months in tarrying upon the coasts, to get knowledge of the princes and countries there.*²⁸

This was a highly secret document; it referred explicitly, for example, to the camouflage publicity about a voyage to Alexandria, so there is no reason to suppose that it falsified the intended course. Unfortunately that part of the document in which the "coast aforenamed" was named has been burned away, so that the question of its identity, which is the key to the whole problem of the objectives, is by no means simple. According to the general maps available to the projectors,²⁹ ships leaving the western end of the strait had the choice of two coasts. One, the imaginary coast of *Terra Australis*, ran south for some five hundred miles, west for perhaps twice that distance, and then continuously northwest until, just south of New Guinea, it reached latitude twenty degrees south. This was certainly not under the obedience of any Christian prince, but to describe a ship following it as sailing "to the northwards" seems extremely loose, if not entirely unreasonable. If *Terra Australis* was the objective, moreover, it is difficult to imagine why Drake should have been limited to exploring as far as thirty degrees, for, according to the maps, Beach and the other desirable places in this southern continent lay at least three thousand miles west of the point at which that coast reached thirty degrees. For these reasons, apart from the fact that the document does not mention *Terra Australis*, Taylor's identification of the "coast aforenamed" appears unconvincing.

²⁸ BM, Cottonian Manuscripts, Otho E. VIII, fols. 8-9. The reconstruction is in Taylor, "More Light on Drake." The putative parts are in italics.

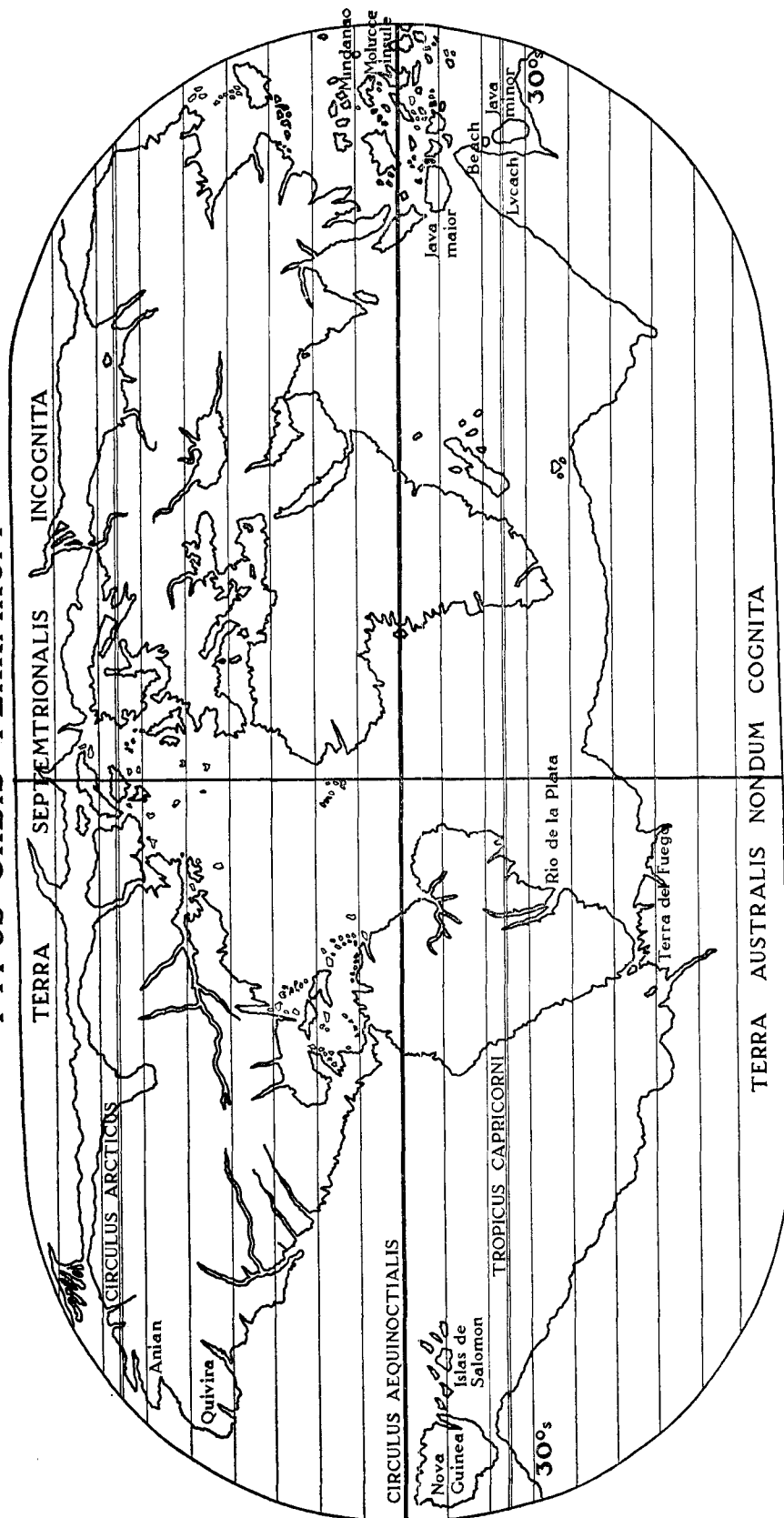
²⁹ Abraham Ortelius' *Theatrum Orbis Terrarum* (Antwerp, 1570) was generally accepted as authoritative in England in the 1570's and was the best atlas available. His world map and his map of America differed as to the trends of the coasts west of the strait, but not radically enough to affect the present argument. Drake was also reported (Nuttall, *New Light on Drake*, 162, 270) to have been using a Portuguese world chart, but it is improbable that this was of much use for the Eastern or even the Western Pacific.

The other coast, the South American, was shown by the maps to run northwest or north northwest. South of Valparaíso, in thirty-three degrees, the Spanish settlements at Concepción in thirty-six degrees and Valdivia in forty degrees were small, isolated, and repeatedly sacked in the later sixteenth century by the fierce Araucanian Indians. The English had no accurate knowledge of the southward extent of Spanish occupation and could reasonably have assumed that Drake would find the coast unoccupied up to some point approaching thirty degrees south, which would explain the proviso "if it shall be thought *meet* by the afore named Francis Drake to *proceed so far*." The "coast *aforenamed*," therefore, could well mean the coast of Chile. Wagner, Taylor, and other writers have already indicated the affinity between Grenville's plans and Drake's, and we have seen that Grenville probably aimed to establish a base in the Plate River and then to reconnoiter the coast of Chile. In the draft plan of Drake's venture the phrase "*like as of the other*" appears to refer to an additional piece of reconnaissance to be carried out before entering the strait, and we may reasonably suppose the coast in question to have been that from the Plate River to the strait, where Drake in fact spent over two months before putting into Port San Julian. The Grenville analogy apart, if it had been intended that Drake should so trespass upon the eastern side, the projectors were not likely to be any more inhibited about the western side, at least as near to thirty degrees as Drake should see fit, or as far as the fringes of Spanish power. Ex post facto there is also repeated reference during the voyage to a rendezvous precisely at thirty degrees south on the coast of "Peru." Finally, we may note that Winter's Admiralty Court statement was headed: "A Declaracion made by me John Winter of a shipp taken by Frauncis Drake Capitaine and generall of five shippes and barckes bound for the partes of America for discoverie and other causes of trade of marchaundizes necessarie and requisite."⁸⁰

For these reasons the phrase "the coast *aforenamed*" seems to fit the coast of Chile far better than *Terra Australis*. Nor is there any hint, in all the documentation of this voyage or Fenton's, that the purpose was to discover that continent. Sarmiento, himself deeply interested in the problem of the unknown land, played a prominent part in the immediate and long-term Spanish reactions to Drake's arrival in the Pacific and wrote about the matter at some length, but it appears never to have occurred to him that the Englishman might be his rival in the quest for *Terra Australis*. Drake did, of course, make an important discovery about that imaginary land. After leaving the strait he was driven south possibly as far as fifty-

⁸⁰ BM, Lansdowne MS. 115, fols. 175-76 (Nuttall, *New Light on Drake*, 383-92).

TYPUS ORBIS TERRARUM



Sketch Based upon the World Map by Abraham Ortelius (1570)

seven degrees, "whereat wee arriveing," says Fletcher, "made both the seas to be one & the self same sea & that there was no farther land beyond the heights of that Iland."⁸¹ The coast of *Terra Australis* must, he concluded, lie much further south. Yet neither he nor anyone else took occasion to relate this discovery to the purpose of the voyage.

One point at least the draft plan makes quite clear: since Drake was to proceed no further than thirty degrees south and then to return through the Strait of Magellan, there was originally no question of visiting the Moluccas or the Strait of Anian. But Taylor has argued that these plans were changed before the fleet left England, the scope of the voyage being extended to include the Moluccas and the Strait of Anian as objectives, the latter possibly as an alternative to the former. Now it cannot be denied that both Drake and Winter departed from the draft plan, but their actions are explicable in terms of the events of the voyage, without introducing the gratuitous assumption that the promoters for some unexplained reason changed their minds as to the general purposes of the venture. The probable reasons for Drake's decision against returning by the Strait of Magellan have already been considered. As for Winter, we may, in view of the draft plan, question even further his claim to have intended a Moluccas voyage. Since the Moluccas were merely an alternative way home from the main objective, and since he was unable or unwilling to carry out the "Peru" plan, his obvious course was to return direct to England. His insistence that he had done all he could to reach the Moluccas must be seen in the light of the essential purpose of his report, which was to convince Burghley that he had not willfully deserted Drake and had made every effort to rejoin him, even to the extent of projecting a voyage halfway round the world. But the rest of the report shows his loyalty in fact to have worn so thin that we may well wonder whether he made any effort at all to persuade his crew to go to the Moluccas and whether, if he did so, it was not merely a charade enacted in anticipation of a charge of desertion.

Did the projectors alter their plan by instructing Drake to search for the Strait of Anian? Taylor cites a passage from Dee's manuscripts to suggest that they did. Writing in mid-May 1577 about the Anian problem, Dee referred to:

that attempt which is by a British subject presently intended. . . . who (God sparing life and health) hath se[cret]ly offered up to God and his natural sovereign and country the employing of all his skill [and] talent, and the patient enduring of the great toil of his body, to that place being the very ends of the

⁸¹ *World Encompassed*, ed. Penzer, 134.

world from us to be reckoned, to accomplish that discovery which of so many and so valiant captains by land and sea hath been so often attempted in vain.³²

Much depends here on the two adverbs, "presently" and "secretly." "Presently," meaning "immediately," would indicate Frobisher, who embarked on his second Northwest Passage venture eleven days later. Drake did not depart until six months later, and it is doubtful whether he had even begun preparations yet. "Secretly" indicates Drake rather than Frobisher, since the latter's project was well known, but the middle four letters of this word are putative. In effect there is room for doubt about Taylor's inference. We know that Grenville had earlier argued the advantages of a Pacific approach to the Northwest Passage,³³ but this does no more than confirm that the idea was available to Drake's promoters.

It may be granted that Drake did make some search for the strait, though the evidence on this point is not conclusive.³⁴ But his strongest conceivable motive for doing so lay in his situation. Having captured the *Cacafuego*, his chief concern was to return by the shortest and safest route, and had the northern passage conformed to the optimistic cosmography of the day, it would have provided a much shorter and, in summer, safer way home than the Pacific route. Nuño da Silva, who was a first-class navigator and had worked with Drake for many months, was convinced that the English would look first for the northern strait and then, if they failed to find it, make for the Philippines.³⁵ Sarmiento, who had more faith in the Northwest Passage than Nuño, insisted that Drake would sail through it rather than risk Iberian opposition in the East Asian archipelagoes.³⁶ We do not know what Drake's intention was at this stage, and he probably hoped to confuse the Spaniards by making mutually contradictory hints. But it is clear that his own situation gave sufficient motive for a northern search, whatever instructions he may have received. Then, having failed to find a strait, he had practically no choice but to return via the Moluccas. Consequently, it is unnecessary to postulate any alteration of the draft plan by Drake's promoters.

³² Taylor, *Tudor Geography*, 116.

³³ BM, Lansdowne MS. 100, fols. 52-54.

³⁴ *World Encompassed* states: "though we searched the coast diligently, even unto the 48 deg., yet found we not the land to trend so much as one point in any place towards the East" (*World Encompassed*, ed. Vaux, 118), but the authority of this 1628 publication is suspect. The "Anonymous Narrative," a better source, says that Drake sailed "northwards till he came to 48 gr. of the septentrional latitude, still finding a very large sea trending toward the north, but being afraid to spend long time in seeking for the strait he turned back again" (*World Encompassed*, ed. Vaux, 183-84). The meaning here is not quite clear, and some other sources, notably the "Famous Voyage," give a different account of the course made good.

³⁵ Nuttall, *New Light on Drake*, 317.

³⁶ *Ibid.*, 74-75, 84-86.

The prevailing interpretation suggests that whereas the majority of the promoters intended a peaceful voyage, Drake reached an understanding with the Queen, secret from all or most of the other promoters, to make the voyage a raid on Spanish shipping and possessions. The notion that Drake and the Queen had their own secret plan is derived from Drake's own statement, reported by Cooke, that Elizabeth and he, brought together by Francis Walsingham, initiated a project to revenge her injuries on the King of Spain by annoying him "in his Indies." Drake also said that the Queen swore that if anyone "did give the king of Spain hereof to understand (as she suspected but two), they should lose their heads therefore." There is no suggestion here that she wished to keep the secret from the other promoters, but in any case the draft plan, which must be accounted a superior source on this point (since Drake's statement was made at a moment of extreme tension in order to assert his authority), shows Drake's story to be false. It lists the subscribers—the Earls of Lincoln and Leicester, Walsingham, Hatton, Sir William and George Winter, John Hawkins, and Drake—and recommends that the Queen be invited to participate and be told "the truth of the voyage." Thus the Queen neither initiated the venture nor at this stage knew its true purpose. Drake's version falls to the ground and with it any inference that he and the Queen had some secret scheme of their own.

Further, the draft project is itself secret so that if there is any truth in Drake's story at all it must have been the draft project that the Queen wished to keep from the King of Spain and Burghley. Such secrecy is perfectly comprehensible if the draft project is seen as a venture to the coast of Chile or "Peru," especially in light of the earlier prohibition of Grenville's similar scheme. Burghley might well have regarded both projects as unwisely provocative. The promoters were all associated with maritime enterprise of a predatory kind, and all, with the exception of the Queen, advocated a vigorously anti-Spanish policy. Elizabeth had presumably been swayed by Burghley's caution to reject Grenville's scheme, but the precise reasons why she evidently changed her mind in 1577 remain to be explained—a task that cannot be attempted here.

Finally, what of Drake's attacks on Spanish shipping and ports? Were these measures not in excess of the draft plan? Clearly they were contrary to the letter of his instructions, but not, we may infer, to their spirit. Encroachment on any part of South America would be seen by Spain as an aggressive act, if not a *casus belli*, and it was probably understood between Drake and his promoters that other forms of aggressive action were permissible, particularly because the operations outlined in the draft plan were

of themselves unlikely to yield immediate profit. Ventures confined solely to reconnaissance were uncommon in Elizabeth's reign, and among Drake's promoters only Walsingham supported that kind of enterprise. Written instructions authorizing plunder, however, are not to be expected. Spain and England were at peace, and there is no reason to suppose that Elizabeth wished to risk war by abandoning her habitual evasion of responsibility for her subjects' anti-Spanish actions. Drake's claim that he possessed some sort of commission from the Queen is not authenticated by any reliable witness.³⁷

The intentions of the promoters can never be certainly known, but the balance of probabilities is such as to suggest that they did not intend the circumnavigation of the globe, nor the foundation of trade with the Moluccas, nor the discovery of the great southern continent; that their project was to reconnoiter the coast of South America from the Plate River round to that part of the coast of Chile where Spanish occupation petered out; and that, knowing this to be a provocative venture, they took precautions to conceal their intentions from Spain and probably tried, though unsuccessfully, to conceal them from Burghley too. They probably assumed that Drake would make the voyage pay by plundering Spanish shipping. The mission of this particular expedition was to establish contact with the American Indians, but a long-term purpose to settle and even perhaps to conquer Spanish Peru cannot be ruled out. Fletcher's narrative contains optimistic remarks about the colonial prospects on both the east and west coasts, and a stronger hint of English ambitions is conveyed in the younger Hakluyt's proposals, drafted in 1579 or 1580 after interviews with some of Winter's men. He advocated the establishment of an English naval base in the Strait of Magellan, with a supporting base at St. Vincent in southern Brazil, the result of which would be "that we shal make subjecte to England all the golden mines of Peru and all the coste and tract of that firme of America upon the Sea of Sur. And work the like effect on the hither side of that Firme."³⁸

Drake did not pervert the venture from its true purposes. He spent over two months on the east coast of America south of thirty-two degrees and a further two at Port San Julian. Having struggled out of the storms west and south of the strait, he spent two months on the west coast from thirty-nine

³⁷ Don Francisco de Zarate declared: "He showed me the commissions that he had received from her and carried" (*ibid.*, 209), but Zarate was probably unable to read English. Nuttall's documents show that Drake was anxious to impress upon the Spaniards that he had the Queen's authority.

³⁸ E. G. R. Taylor, *The Original Writings and Correspondence of the Two Richard Hakluyts* (London, 1935), 139-46.

degrees south to twenty-eight degrees south. His decision then to set course north contravened the letter of his instructions, but was fully justified by the dangerous weather conditions on the recommended return route and by the generally unpromising results of his reconnaissance. Neither this decision nor Drake's general procedure in the Pacific was the result of any secret plan concocted by the Queen and himself. In an ambitious expedition of this sort to seas previously unvisited by English ships the commander had to be left considerable discretion to change the planned itinerary according to circumstances. In what survives of the draft plan Drake is empowered to decide the delicate question of how far north to proceed along the coast of Chile, and it is inconceivable that he was not allowed to use discretion in other respects. Once he was clear of the strong westerlies two possibilities for the return route presented themselves: the Moluccas and the Strait of Anian. The probability is that he entertained both ideas and did not make up his mind finally in favor of the Moluccas until he had become convinced that the opening of the Strait of Anian lay too far north for an easy passage home. Arriving in the East Indies, he did what he could for English commercial interests, but achieved much less than he might have done had he been suitably prepared to treat this as a major object of the expedition.³⁹

³⁹ This interpretation incidentally implies that the annexation of New Albion was not part of the original plan, and what is known of the episode itself suggests that Drake's claiming of that land for the Queen was a characteristic piece of opportunism. There was no question of settlement then and there, and even a future colony must have seemed a remote possibility. Drake is reported as "wishing nothing more than that it had lain so fitly for her Majesty to enjoy as it was now her proper own." That he had in mind its potential value as a base should the Strait of Anian be found is a reasonable hypothesis, but there is no positive evidence to this effect. Nuttall's notion of "Drake's dream" hardly requires comment since it has been sufficiently discredited by Wagner.

The Myth of the China Market, 1890–1914

PAUL A. VARG*

HISTORIANS have amply demonstrated the widespread interest in foreign markets following upon the growth of manufacturing and the closing of the frontier. This article tests the rhetoric of the generation between 1890 and 1914 against the realities of the China market for American goods and against the specific efforts of both business and government to capture it.

We need not cover in any detail what other scholars have already covered. Great numbers of Americans did much writing and even more talking about the future importance of China as a market for American goods. Journalists told of 400,000,000 customers and how, if they added but one inch to their frock coats, the textile mills of New England would be kept busy for years to come.

Historians have documented the far-reaching hopes of Americans. Charles Campbell in his *Special Business Interests and the Open Door* (New Haven, Conn., 1951) gives an account of the American Asiatic Association, a group of merchants engaged in the China trade, and their extravagant hopes during the late 1890's. Julius Pratt in his *Expansionists of 1898* (Baltimore, 1936) discusses the interest in the China market during the same decade, and Walter LaFeber in *The New Empire* (Ithaca, N. Y., 1963) likewise deals at length with the business community's discussion of the importance of China as an outlet for surplus goods. Although these accounts terminate with the decade of the 1890's, an extreme optimism prevailed for the next several years. The American Asiatic Association promoted the cause in political councils throughout the first decade of the century. At the same time, the weekly New York newspaper, the *Journal of Commerce and Commercial Chronicle*, carried forward the campaign.

It flattered the ego of Americans to think of their country as the supplier of the world's market and coincided with their nationalistic spirit. It likewise seemed to promise a solution to the problem of the much-discussed surplus. Why this interest focused on China is not clear. To be sure, exports to that country increased sufficiently in the 1890's to provide some basis for

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the optimistic predictions, but this optimism should have been tempered by recognition of the fact that China scarcely absorbed 3 per cent of the nation's exports. The optimism might even have been wholly dissipated by greater familiarity with the obstacles facing trade in China.

The most elementary facts contradicted the dreams that China would before long provide a large market. First, only a small part of China, the coastal cities and a few ports on the rivers, was open to trade. In 1899 Rounseville Wildman, the United States consul general at Hong Kong, wrote: "Another great point that American exporters overlook is that 99 percent of China is still closed to the world. When the magazine writer refers in glowing terms to the 400,000,000 inhabitants of China, he forgets that 350,000,000 are a dead letter so far as commerce is concerned."¹ In 1901 Burlingame Johnson, consul at Amoy, called for treaty revision in order to permit businessmen to reside in the interior. This, he believed, would open the markets as far away "as 150 to 200 miles . . . whereas now even kerosene and flour seldom get further than fifty miles from open ports and few other goods that far."²

The lack of a transportation system restricted the influx of Western goods. Except for river traffic, the system was almost nonexistent. The Grand Canal had at the time of Marco Polo's sojourn carried large vessels some 650 miles, but by the late nineteenth century it was in disrepair, and small junks navigated it with difficulty. Of the roads, the United States consul at Shanghai in 1895 reported: "Their condition is such that passage over them is virtually stopped, as the holes and ruts that deface them force travelers to desert them for the tracks by the sides, although these in wet weather are but quagmires, and in dry weather, several inches in dust."³

A survey of the roads in 1890 by the China branch of the Royal Asiatic Society concluded:

Probably no country in the world, certainly none aiming at civilization even of the most rudimentary nature, has paid so little attention to roads and means of communication as has the Chinese empire; and it may be remarked at the outset that no road in the European acceptance of the term, as an artificially constructed viaduct, laid out with engineering skill even of the crudest description, exists from one end of China to the other.⁴

¹ Report of consul general at Hong Kong, Wildman, *Commercial Relations of the United States with Foreign Countries during the Year 1899* (2 vols., Washington, D. C., 1900), I, 874.

² Johnson to David J. Hill, Assistant Secretary of State, Mar. 20, 1901, Department of State Archives. The microfilm copies of these dispatches and the dispatches from and the instructions to the United States legation in Peking are available in the library at Michigan State University.

³ *Special Consular Reports. Highways of Commerce. The Ocean Lines, Railways, Canals, and Other Trade Routes of Foreign Countries* (85 vols., Washington, D. C., 1895), XII, 597.

⁴ *Ibid.*, 600.

Given this condition, only a small part of the country was accessible to foreign goods.

Another formidable barrier stood in the way: Western manufactures fitted neither ancient Chinese preferences nor Chinese pocketbooks. Flour, cotton goods, kerosene, and lumber fit in with the native consumer habits and did find a growing market, but the great variety of Western goods ran counter to long-established methods and customs. In 1906 James L. Rodgers, the consul general at Shanghai, wrote: "It is perhaps needless to call attention to the antiquity of Chinese methods and habits, to the fact that traders have for centuries been trying to introduce new things, and that beyond some modern devices for using and making the necessities of life one sees very few inroads upon established customs." Rodgers stated that the Chinese did buy foreign oil, flour, leather, lamps, clocks, and some foodstuffs, but, he warned, "it does not follow that there is a market for a foreign shoe, for an agricultural implement, for machinery of various kinds and for the infinite variety of manufactured goods which distinguish the industry of the United States, Great Britain, and Germany." Some Westerner, Rodgers reported, had written home that there was a great market for windmills. Such an opinion ignored the fact that the Chinese had been raising water from one level to another by means of pumps and water wheels long before the Christian era began, and they were not likely to change. Even more important than the reluctance to change, he declared, was the fact that "a windmill would cost many rice crops, or perhaps the savings of a lifetime. . . ."⁵

Rodgers, after examination of the markets of the Chinese cities near Shanghai, noted that there were few foreign products, and "you will hunt for a day before you will find in this section of China an agricultural implement of foreign make."⁶ He concluded: "Numberless instances might be cited to show how limited a Chinese market is for things which encroach upon their customs or which will supplant the articles handed down from generation to generation. . . ." In conclusion he offered these words: "And all this is written not to discourage but to place that which is conceived to be plain truth before the minds of those who nowadays read in the newspapers glowing prophecies about the oriental trade, who then remember that there are said to be four hundred million Chinese and who are straightway moved to attempt an export business to China. . . ." Given the "present scheme of civilization" whereby the Chinese "are practically sufficient unto themselves," he warned, "China even under the reformation now beginning

⁵ Rodgers to Department of State, Jan. 8, 1906, Department of States Archives.

⁶ *Ibid.*

will take at first only in a small way of those things she does not seem to need. . . ."⁷

The poverty of the Chinese constituted a further obstacle. When the Department of State in 1898 instructed consuls throughout the world to report on the possible outlets for the surplus product of soap manufacturers, E. T. Williams, then vice-consul general at Shanghai, wrote:

The people of China are extremely poor. Their wages are paid in copper cash, one of which equals one-twentieth of a cent. One hundred to one hundred and fifty of these cash, that is, from five to seven and a half cents, form the average daily wage of the ordinary working man. It is evident that such an article as soap, which from the Chinese point of view is an article of luxury rather than necessity, however much desired, can be purchased only when furnished at a very low price.⁸

As Gerald Winfield was to put it five decades later in his book *China: The Land and the People* (New York, 1948), the poverty that prevailed had as its symbol the family pig. Other livestock was extremely rare because feeding entailed a loss of nutritional elements, but the pig, living in the family latrine, required almost no feeding.

Other countries emerging from an almost wholly agricultural economy turned natural resources and an abundant supply of cheap labor into assets through rapid industrialization. Japan is a noteworthy example. In China, however, the change was slow. The central government lacked the necessary revenue to foster industry with any degree of rapidity, and Chinese social values, especially the tradition of investing any surplus saving in land, handicapped the development of industry. The custom of "squeeze," nepotism, and the use of government-owned enterprises for personal political ends likewise stood in the way of economic development.

Another deterrent to establishing a market in China for American goods was the rapid development after 1894 of an unfavorable trade balance. Exports to China did increase, but exports from China did not. A study of China's long-term trade developments made by the Imperial Maritime Customs in 1904 showed that China's imports had increased until they were a third greater than exports. Indemnities incurred as a result of the war with Japan and the Boxer rebellion had necessitated foreign loans thereby increasing the outflow of gold. These foreign loans, in 1904, called for payments of more than hk. tls. 45,000,000 a year.⁹ This financial situation would ultimately handicap sales to China.

⁷ *Ibid.*

⁸ *Special Consular Reports* (85 vols., Washington, D. C., 1898), XVI, pt. 1, 35.

⁹ *China Imperial Maritime Customs*, 11- Special Ser., No. 11, An inquiry into the commercial liabilities and assets of China in international trade, published by order of the inspector general of customs (Shanghai, 1904).

Within this China market, so circumscribed by inaccessibility to the interior, by aversion to Western-style manufactures, by poverty, and by an unfavorable balance of international payments, a fiercely competitive struggle for sales and contracts raged. Germany, Great Britain, France, Russia, and Japan were more dependent upon foreign markets than the United States; the governments of these nations gave their business enterprises greater support than did the US; and, most important, the business enterprises of these countries demonstrated greater energy and initiative in China. Consequently, American companies encountered difficulties. Price competition was particularly severe.

The Standard Oil Company, oriented to foreign markets by long experience and by the fact that since the 1860's more than half of its major product, kerosene, was exported, eyed the Far East. As early as 1882 the company sent William Herbert Libby to explore possible markets there. He made a careful study of the China situation and, more particularly, of the barriers to greater sales of kerosene. Beginning in 1890 Standard Oil, anxious to expand sales, departed from the practice of selling to merchants on the Atlantic seaboard who then handled sales in China. Under the new system it distributed its products through its British affiliate, the Anglo-American Oil Company.¹⁰ In the next two decades sales increased, but Standard Oil's hopes of dominating the market never came even close to realization. Russian oil enjoyed the advantages of lower production costs, shorter transportation routes, and benefited by the tariffs levied on value as opposed to volume.¹¹ The competition of the Dutch operating out of the East Indies also cut seriously into Standard Oil's sales in China. Standard Oil, more than any other American company, adopted a system of distribution and sales that was efficient and well suited to success in China, but although sales became important, the competition of the Russians and the Dutch was so effective that, in the words of the historians of the company, its "efforts in the Far East proved relatively ineffectual."¹²

These barriers to trade, although not readily surmountable, sometimes appeared minor in comparison to inveterate Chinese hostility toward the foreigner. The one characteristic quality of the Chinese in relations with the outside world whether political, economic, or religious, was, indeed, an intractable opposition. The missionaries, more often exposed to antagonism because their efforts touched upon matters subject to deep emotional response and because they were often in the interior, were the most frequent

¹⁰ Ralph W. Hidy and Muriel E. Hidy, *Pioneering in Big Business* (New York, 1955), 152.

¹¹ *Ibid.*, 132-33, 259.

¹² *Ibid.*, 267.

targets of antiforeign disturbances. Business and government representatives enjoyed the protection of treaty ports, but they could not be protected from the Chinese aversion to them that found expression in delayed negotiations, the placing of obstacles in the way of land purchases, and the playing of one foreigner against another.

Samuel F. Gracey, consul at Foochow, reported that after fifteen years of carefully studying all ranks of Chinese officials "and all grades of Chinese, from the above-named, to the coolie class, I am persuaded that, speaking broadly, all are unfavorable to foreigners."¹³ Gracey concluded that they were convinced of the superiority of their own way of doing things, and they resented the intrusion of the foreigner. The official classes, wrote Gracey, "resent the coming into the country of these meddlesome foreigners." "They find the ground slipping from under them by the impact of Western civilization, which is forcing upon them reforms, the trend of which is all toward great changes in their cherished beliefs, customs, learnings, and methods; and they cannot see whereunto all this is leading them," wrote Gracey. The literate and gentry, he stated, "would only be too happy to sweep them all out of the empire"¹⁴

At times hostility toward the foreigner became particularly pronounced. During the early 1890's, 1900, and the period immediately after the Russo-Japanese War this antagonism was most obvious. In February 1906 the United States consul at Hankow reported: "It would almost seem as though there was a concerted plan to do nothing the foreigner wants, but as far as possible to place every conceivable obstacle in the way of securing the rights pledged him under the treaty. . . ."¹⁵ In June he reported that there was a quiet but deep hostility toward all foreigners among the seven thousand to ten thousand coolies employed by the shipping interests and tea factories in Hankow. "They are like wolves," he wrote; "one will slink away but a number of them will attack with their carrying sticks."¹⁶

The government at Peking, bending before superior force, signed treaties that seemingly opened China to Western business, but the Chinese illustrated the human genius for resisting by indirect methods what cannot be undone by direct confrontation. Foreign consuls railed against this trait as a perversity peculiar to the Chinese. Gracey at Foochow negotiated for six months with the viceroy and the Foreign Board for the purchase of land for the use of the Standard Oil Company. "They have shown constant

¹³ Gracey to Robert Bacon, Assistant Secretary of State, Dec. 6, 1906, Department of State Archives.

¹⁴ *Ibid.*

¹⁵ William Martin, consul at Hankow, to Bacon, Feb. 6, 1906, *ibid.*

¹⁶ Martin to Bacon, June 30, 1906, *ibid.*

purpose to defeat us by presenting unusual obstacles," he reported. The latest device was to insist on the sellers appearing at the yamen. "It is well known, that in many parts of the Empire, when sellers of land were required to go to the yamens they were not only unmercifully squeezed, but in many cases were beaten, for selling to foreigners, and it became so common that no persons could be found to sell to foreigners, thus defeating treaty provisions," Gracey informed the Department of State.¹⁷ Given this marvelous exhibition of Chinese recalcitrance, even the most formidable advocates of American economic expansion inevitably had to recede.

Chinese resistance to foreign control made the situation seem futile at times to those bent on opening up China to trade and development. No representative of the United States put the problem as tersely as E. H. Conger, minister to China. It was, he said, a question of two choices. China could be left to itself and allowed "to proceed alone and in the same way that she has for thousands of years, with her inexhaustible resources of material and trade still undeveloped; or her mines must be opened and her railways built and trade developed by foreigners."¹⁸ In the latter case there would be stubborn opposition "which the Chinese government will be unable to promptly and satisfactorily suppress. . . ." He saw this as so formidable a threat to the interests of Western nations that he was led to ask: "It is true the integrity of China can easily be preserved by an alliance of a few of the great powers, but to what end?"¹⁹ The intense hostility of the Chinese customer served to circumscribe the market.

Turning to a second major aspect of the problem, the willingness of the government in Washington to lend assistance to American business, we find that the support was usually little more than an expression of good will. Beginning in the late 1880's the Department of Commerce and the Department of State were vigorous in asking consuls for reports on commercial opportunities for many items of manufacture. Bulletins including the reports were issued in great numbers.

Since both Charles Denby and Conger believed that the investment of American capital would spur the sale of American goods, they supported their fellow countrymen when they presented proposals. In April 1898 Denby reported to Secretary of State John Sherman that he had "devoted a great deal of time and labor to the promotion of railroad projects" presented by his countrymen.²⁰ His successor, Conger, later in the same year wrote: "So long as I am at this Legation, its aid will be cheerfully and

¹⁷ Gracey to Bacon, Dec. 11, 1906, *ibid.*

¹⁸ Conger to Secretary of State John Hay, Nov. 3, 1898, *ibid.*

¹⁹ *Ibid.*

²⁰ Denby, minister to China, to Secretary of State Sherman, Apr. 15, 1898, *ibid.*

actively given along these lines so far as is wise and proper; but experience has long since proven that neither legislation nor official aid can take the place of business enterprise in business affairs."²¹ It was also true "that one of the chief elements of foreign potency, is the leverage obtained from actual occupation or ownership of territory." This was Conger's observation in August 1898.²² During the next few years, the reverse was also true on occasion. Americans received some advantage because their schemes were considered to be free of political ambitions. In the period 1894-1906, Washington, through its representative in Peking, struck its boldest pose when the Chinese government canceled the contract of the American China Development Company for building the railroad from Hankow to Canton. The cancellation led to sharp diplomatic notes and the minister, W. W. Rockhill, questioned the chief of China's Foreign Office in a most peremptory tone.²³

The promotion of economic interests was, however, generally the function of the consular service rather than the legation in Peking. If the degree of government support of the consular service is a fair measure of how seriously Washington took the promotion of interests in China, the conclusion can only be that interest approximated apathy. For years the consul general at Shanghai, Rodgers, protested that the American consular offices in that city were not only inadequate but reflected unfavorably upon the United States. As late as 1905 he declared that they were the poorest of any foreign nation except Portugal. There was also a very small staff. Rodgers compared the failure to provide an adequate group of consular officers with the elaborate efforts of Great Britain, Germany, France, and Japan. In September 1905 he reported:

They know for instance that Great Britain has a force of Englishmen in the various departments of its representation; that Germany has not only a large number here, but also has men traveling on trade matters; that France is likewise provided and that Japan is represented elsewhere. They know that absolute count will show that in Shanghai where the United States has one employee, Great Britain and Germany have six, France about four and Japan counting only those in evidence three.²⁴

All of the consular districts found reason to complain, but no consul demonstrated greater impatience than Edward Bedloe who was appointed consul at Canton in the latter part of 1897. On his arrival he found the

²¹ Conger to Secretary of State William R. Day, July 31, 1898, *ibid.*

²² Conger to Day, Aug. 26, 1898, *ibid.*

²³ An account of this episode is given in Paul A. Varg, *Open Door Diplomats: The Life of W. W. Rockhill* (Urbana, Ill., 1952), 72-76.

²⁴ Rodgers to Francis B. Loomis, Assistant Secretary of State, Sept. 14, 1905, Department of State Archives.

offices so inadequately furnished that he carried on his business from his hotel room. When facing the necessity of giving a reception in the consular offices for Chinese officials and other consuls, he borrowed furnishings from several friendly parties.²⁵

The importance of the Canton consular district seemed to justify better quarters and a more adequate staff. Eighty million people lived in the area. Seven cities had been made into treaty ports in 1897, and both the British and Germans had a consular official at each. Consul Bedloe was the only officer representing the United States. When he first took over, he had no vice-consul and no clerk. During his first several months he employed a clerk and paid him out of his own pocket. A vice-consul was appointed late in 1898 after a missionary group petitioned the Department of State.²⁶ The inadequacy of staff, particularly the absence of consuls in the interior, meant that there was no official to protest against a variety of types of interference with shipments of American goods or to promote American commercial interests.

The United States consulate at Amoy typified the general neglect and apathy. In the early 1890's Bedloe, previous to his transfer to Canton, occupied the office. A German resident served for several months after Bedloe was transferred. Then Delaware Kemper took over. In June 1897 Burlingame Johnson, an energetic young man from Colorado, replaced Kemper. Johnson immediately reported to the Department of State "that the condition in which the work of the office has been found is very unsatisfactory." The "property," he declared, "is in a most dilapidated condition." He added: "The verandas are falling, posts have rotted off, platings [*sic*] falling, and the roof needs thorough repairs."²⁷ An official reading the letter noted: "He may have the flagstaff painted at once." Within a year seventeen hundred dollars was spent on renovations.

The work of the Amoy consulate harmonized with the dismal surroundings. Johnson informed the Department of State: "Notwithstanding this I find that absolutely no attention was given to the opening for American products by my predecessor and that for three years there has not been a single trade report to the Department calling attention of exporters to existing conditions."²⁸ Johnson's initial enthusiasm found expression in a detailed report on missionary work, praising it as a philanthropic effort and as an activity that opened the door to commerce, but his efforts in behalf of trade do not appear to have measured up to his own high hopes.

²⁵ Bedloe to Day, July 7, 1898, *ibid.*

²⁶ Bedloe to Day, Feb. 10, 1898, *ibid.*; see also Bedloe's letters of Apr. 11, July 18, 1898, *ibid.*

²⁷ Johnson to Department of State, June 26, 1897, *ibid.*

²⁸ Johnson to Department of State, Aug. 4, 1897, *ibid.*

In April 1906 the consul at Hankow, William Martin, complained "that all the force in this office at present, capable of doing clerical work, consists of Mr. W. B. Hull, Student Interpreter, Mr. Kong Chen-ren the Chinese writer and myself."²⁹ He asked for a stenographer and a typewriter, basing his request on the sharp increase of Standard Oil's business, but acknowledging that his plea had a more important basis in the great numbers of missionaries scattered over the district and the voluminous correspondence carried on with them.³⁰ In 1902 Samuel Gracey, after many years of service, requested restoration of his salary to what it was previously: \$3,500.00. John Fowler, a veteran consul stationed at Chefoo, one of the more important posts from the point of view of sale of cotton goods, received a raise to \$3,500.00 in 1905. He noted: "it is the smallest salary any professional Consul or Vice-Consul is receiving at this port, and all of my colleagues in course of time will retire on a pension larger than the salary of \$3,500."³¹ Fowler, a short time later, protested that his allowance of \$1,775.00 for contingent expenses fell far short of the average annual \$3,209.85 contingency expenses during the previous five years. He met the difference by dipping into his own pocket.³²

This penurious policy resulted, of course, in a rapid turnover of personnel and in much incompetence. The interest in foreign markets led to agitation for reform, but there was long delay because appointments to consular posts were an important source of patronage for members of Congress. Not until 1906 did Congress provide for improvements. In a final speech in the House of Representatives supporting the bill, Robert Adams of Pennsylvania cited the fact that for "sixteen years efforts have been made to secure the proposed legislation." "The new legislation," he agreed, "will go a long way in the movement that is now occupying the time of our merchants for the enlargement of our foreign commerce, for these are our advance pickets, sent throughout the world to furnish the merchants the necessary information to enlarge their business abroad."³³

The new law establishing five categories of consular posts based on estimates of the commercial importance of the foreign city did indicate a degree of serious purpose concerning China. Shanghai and Hong Kong were placed in the second category, Tientsin and Canton in the fourth, and Amoy and Foochow in the fifth among the consul general posts.

The improved consular service reflected the government's increasing

²⁹ Martin to Department of State, Apr. 5, 1906, *ibid.*

³⁰ *Ibid.*

³¹ Fowler to Hay, May 29, 1905, *ibid.*

³² Fowler to Assistant Secretary of State Loomis, Aug. 11, 1905, *ibid.*

³³ *Congressional Record*, 59 Cong., 1 sess., XL, pt. 4 (Mar. 19, 1906), 3975.

awareness of the importance of foreign trade. The importance of export markets in the eyes of Washington is also evidenced in the strong support given to economic interests in Cuba, Santo Domingo, and the Philippines after the war with Spain. The building of the Panama Canal was likewise in part an extending of the helping hand of government to commercial interests. Others have discussed the role of economic considerations in the move of the Taft administration to neutralize the railroads of Manchuria. They were important, but compared to the actions of some other governments, Washington scarcely played the game in a daring manner.

We are here dealing with the market for goods rather than for investment, but the former cannot be treated without some reference to the latter. The lack of investments, especially in railroads, was quite correctly viewed by contemporaries as one of the reasons why the sale of American goods was not greater.

The situation as it had developed in 1909 was well described by C. S. Donaldson, chief of the Consular Division of the Bureau of Manufactures. He explained how the bankers of Germany, France, and Great Britain, with the support of their governments, established banking houses in China, South America, and elsewhere and then "turned all the trade possible to their nationals."³⁴ Against these "tripartite combinations of government, banker and the manufacturing exporter, the American seeking trade abroad has contended single-handed." The US, he said, was just beginning to lend a hand, and he cited the work of individual diplomatic representatives, the effect of the government's handling the customs revenues of the Dominican Republic, and the participation of the Department of State in refunding the twenty-million-dollar debts of Costa Rica and Honduras. The Bureau of Manufactures was providing useful information to exporters, and consuls were likewise to be credited with "useful achievements." The author praised the consuls. Yet, while a new energy was making itself felt, Donaldson warned: "We can only try to make up in activity what these competing nations accomplish through associating public and private interests in strengthening their economic position."³⁵

The policy of government aid certainly rested on wide agreement on the importance of exports, but its implementation fell somewhere beyond half-heartedness and considerably short of boldness. Wide agreement did not produce aggressiveness because the very economic interests that might be expected to spur government action were now concerned with other

³⁴ C. S. Donaldson, "Government Assistance to Export Trade," *Annals of the American Academy of Political and Social Science*, XXXIV (Nov. 1909), 555.

³⁵ *Ibid.*

matters, such as expanding the tremendous home market and gaining tariff protection. In 1909 John Barrett, director of the Bureau of American Republics, in an address before the National Association of Manufacturers, bemoaned the fact that in all the speeches in Congress over the new tariff bill, and in almost all the discussions in the newspapers, "there has been an absolute neglect of the effect the tariff may have on our export trade."³⁶ In brief, in spite of a consensus of opinion on the importance of foreign trade, government action was moderated by concerns that evoked a much greater response.

Having examined both the strengths and weaknesses of the China market at the turn of the century and the degree of support provided by the government in Washington in efforts to capture this market, next I will discuss whether the American business community demonstrated energy and imagination in China. Some of the consuls stationed at major ports took a deep interest in the business activities of their fellow nationals, and they prepared lengthy reports and wrote frequent letters containing detailed observations on commerce, the opportunities at hand, the factors making for success and failure, and the nature of the competition. In the 1890's a majority of them filed optimistic reports and heralded even minor advances in sales of American goods, but throughout the hundreds of these reports and letters there is common complaint of the lack of assertiveness on the part of American business concerns.

The apathy of American business showed itself in a variety of ways. In Chefoo, Fowler complained of the failure of American concerns to provide credit facilities, of the failure to send representatives to promote sales, and of the poor packaging of American goods.³⁷ These practices did not change. Eleven years later, in 1911, George Anderson, in charge of the consulate at Hong Kong, attributed the decline in sales in recent years to the high prices of American goods, Japanese competition, failure to supply credit, poor packaging, and the lack of an effective sales organization.³⁸ Another consul cited the failure of Americans to invest in China and reminded his readers that trade follows investment.³⁹ Willard B. Hull, vice-consul general at Hankow, warned that American firms could not follow their present policies and hope to secure business. "Nearly every American

³⁶ John Barrett, "South America—Our Manufacturers Greatest Opportunity," *ibid.*, 521.

³⁷ *Consular Reports, Commerce, Manufactures, Etc.* (75 vols., Washington, D. C., 1899), LIX, Nos. 220, 221, 222, 223, 550-51.

³⁸ George Anderson, *Cotton-Goods Trade in China* (Washington, D. C., 1911), 14, 16, 17, 30.

³⁹ Consul general Charles Denby, "Review of Trade Conditions of China," in *Commercial Relations of the United States with Foreign Countries during the Year 1907* (2 vols., Washington, D. C., 1908), I, 345.

company represented in Hankow," wrote Hull, "has some European firm for its agent, and, naturally, American products will be sold only when these firms cannot secure the same things from their own country in Europe, thus keeping American goods, in most cases, as a second choice." Hull likewise advised that American manufacturers "must also count on giving longer credits if they wish to do business in this field."⁴⁰ Percival Heintzleman, vice-consul general at Shanghai, in 1908, stated that the three greatest handicaps of United States trade were: failure to extend credit; failure to send representatives; and failure to invest American capital.⁴¹ The vice-consul at Dalny, in 1909, deplored the failure to send representatives. American business, he observed, was in the hands of persons who were regarded as commercial rivals.⁴²

American business, with the notable exception of Standard Oil, made no great effort to do what was necessary to sell to China. One major reason seems to have been the greener pastures near at hand. Anderson reported:

They state frankly here that the cotton-goods market in the United States is so great, its demands so steady, the prices it pays so good, and its consumption so broad that American manufacturers will give no more than passing interest to any foreign market and will not make the effort necessary to secure foreign business until home conditions turn against them.⁴³

These observations of consular officers lead to the conclusion that American business was apathetic or at least unimaginative in its methods.

United States ministers at Peking often expressed regret over the lack of enterprise. In October 1897 Denby observed: "Unfortunately, our fellow-citizens have made no serious effort to avail themselves of the good will of China." Two years before a loan of \$100,000,000.00 had been offered to Americans, but he recorded: "I could find nobody in the United States that would touch it." American banking representatives had gone to China, but they were without authority to make a contract. Denby advised: "To accomplish anything here we must imitate the European powers and have fully authorized agents on the ground."⁴⁴

Denby's successor, Conger, reported that Europeans were active in

⁴⁰ Willard B. Hull, "Hankow," *ibid.*, 378.

⁴¹ *Commercial Relations of the United States with Foreign Countries during the Year 1908* (2 vols., Washington, D. C., 1909), II, 413-14.

⁴² *Commercial Relations of the United States with Foreign Countries during the Year 1909* (Washington, D. C., 1910), 739. The vice-consul at Dalny was Adolph A. Williamson. He wrote: "This decline of American trade is in part due to the absence of American effort, coupled with the energy of English and Japanese competitors, the Germans showing more interest toward the close of the year. There are no American houses established here and the agencies are in the hands of persons who are regarded as commercial rivals. Under such conditions not much increase of American trade is to be expected."

⁴³ Anderson, *Cotton-Goods Trade in China*, 30.

⁴⁴ Denby to Secretary of State Sherman, Oct. 20, 1897, Department of State Archives.

studying opportunities for railroads and mines: "If our capitalists really desire a share they must have brains and money here."⁴⁵

The apathy of American business in the China market did not correspond to their behavior elsewhere if we may assume that success in sales was a result of their initiative. Exports of domestic manufactures increased dramatically. In 1890 they constituted only 12.48 per cent of total exports; in 1900 they represented 31.65 per cent of the total.⁴⁶ In 1910 the value of exported manufactures reached \$767,000,000.00 compared to \$122,000,000.00 in 1880.⁴⁷

An examination of figures on the China trade shows that it was limited to a very few commodities. Illuminating oil and cotton goods led the way by a wide margin. Tobacco and tobacco products ranked third, and lumber was fourth. Analyzing these further, we find that unbleached cloth constituted most of the textiles. In the peak year, 1909, unbleached cloth exports totaled \$6,983,774.00; bleached cloth was valued at \$908,681.00 and colored cloth at \$111,402.00.⁴⁸ The total exports of these three varieties in 1910 were \$10,093,985.00 of unbleached, \$1,351,040.00 of bleached, and \$8,521,466.00 of colored; of the total China took \$5,762,318.00 or approximately 27 per cent.⁴⁹ Cotton textiles, however, ranked eleventh among the exports of the United States in 1910 and accounted for only 1.95 per cent of the value of all exports.⁵⁰

Sale of illuminating oil totaled \$1,251,201.00 in 1900, reached a peak for this period of \$8,499,279.00 in 1908, and declined again to \$5,015,397.00 in 1910. In the last year, total exports of illuminating oil were valued at \$62,477,527.00, and the Chinese market accounted for 8 per cent.⁵¹

The next most important item in the trade fell far below cotton cloth and oil. Exports to China of leaf tobacco amounted to \$639,369.00 in 1906, dipped to \$273,687.00 in 1909, and advanced to a peak of \$653,496.00 in 1910.⁵² Exports of cigarettes reached a high of \$1,393,051.00 in 1907 and then slipped to \$793,381.00 in 1908.⁵³ The chief lumber products exported to China were boards, deals, and planes. They totaled \$975,629.00 in 1907, but declined by 50 per cent in 1909 and then recovered in part, amounting to \$748,026.00 or 2 per cent of total exports of these lumber items in 1910.⁵⁴

⁴⁵ Conger to Day, July 31, 1898, *ibid.*

⁴⁶ *Statistical Abstract of the United States* (Washington, D. C., 1901), 187.

⁴⁷ *The Foreign Commerce and Navigation of the United States for the Year Ending June 30, 1910* (Washington, D. C., 1910), 19.

⁴⁸ *Ibid.*, 544-48.

⁴⁹ *Ibid.*

⁵⁰ *Ibid.*, 58.

⁵¹ *Ibid.*, 692.

⁵² *Ibid.*, 738.

⁵³ *Ibid.*, 740.

⁵⁴ *Ibid.*, 756.

These major exports represent most of the trade: \$13,003,470.00 of a total of \$16,181,670.00 in 1910. Sales of other important items of manufactures were either trivial or nonexistent. Railway cars, carriages, and other equipment varied, totaling only \$382.00 in 1906, mounting to \$137,439.00 in 1909, and then falling to \$17,204.00 in 1910.⁵⁵ Sales of railway equipment to Japan in its sphere in China were greater. The rebuilding of the South Manchurian Railway, destroyed by the Russians during the war, was done largely with American-made equipment, and in 1908 the sales totaled almost \$2,000,000.00. Rails, considered a separate item, were sold to Japan for use in China to the extent of \$1,121,199.00.⁵⁶ But, in the case of both equipment and rails, sales were trivial in most years. Locomotives, also considered a separate item, were sold in large quantity (\$2,404,619.00 in 1910) in one year and scarcely any in most years.⁵⁷

The point that sales of most items of manufacture to China were small is well illustrated by the statistics for 1900. In that year American manufacturers sold \$292.00 worth of cash registers, \$6,345.00 of electrical supplies, \$2,102.00 of laundry machinery, \$17,520.00 of pumps and pumping machinery, and \$7,769.00 of sewing machines. These were not the only items sold, but they are representative. Obviously, they were scarcely adequate to enliven the interest of manufacturers.

Contemporary observers of China trade saw that the availability of credit and investment of American dollars was necessary for increasing sales. Recognition of this interdependence of trade and investment eventually encouraged bankers to show an interest in China, but they found American opportunities and a few selective foreign ones more promising. The domestic demands for capital in a country of burgeoning industrial growth absorbed most attention. The fact that the United States remained a debtor nation until World War I was, of course, of primary importance in explaining the absence of American capital in China.

Yet a transition was under way. Reviewing financial developments during 1900, the editor of *The Commercial and Financial Chronicle* noted that American bankers had been able to relieve Europe's loan requirements on several occasions. It was, he said, the first time that European governments turned to the United States for such help. He described this new development as marking "an epoch in American history."⁵⁸ After 1900 investments abroad showed significant gains.

⁵⁵ *Ibid.*, 518.

⁵⁶ *Ibid.*, 608.

⁵⁷ *Ibid.*, 627. For more specific facts concerning Japan's purchase of railway equipment in the United States for the rebuilding of the South Manchuria Railway, see report by Roger S. Greene, consul at Dalny, in *Commercial Relations of the United States with Foreign Countries during the Year 1907*, I, 360.

⁵⁸ Editorial, *The Commercial and Financial Chronicle*, Jan. 5, 1901.

This change did not direct itself toward China except in a minor way. In his study of foreign investments in China, C. F. Remer estimated the value of American business holdings in Shanghai in 1900 at \$10,000,000.00 and the value of American business holdings throughout China at \$17,500,000.00. Remer found that by 1915 the value of American investments in China was about \$42,000,000.00. British, Japanese, Russian, and French investments far surpassed those of Americans.

Two ventures marked the beginning of a serious purpose of promoting both trade and political influence by means of investment. In 1898 the American China Development Company secured the contract for the building of a railroad from Canton to Hankow. Not long after negotiation of this contract, control passed to a Belgian syndicate, but in 1904 a group of New York capitalists purchased a controlling interest. At this point Chinese resentment over the poor performance of the company and a new determination to control its own railroads led to cancellation of the contract. Because the government in Peking was willing to pay an exorbitant price, the American bankers accepted the cancellation in tranquil spirit, but not so President Theodore Roosevelt and Secretary of State Elihu Root.⁵⁹ They viewed the sale as a serious blow to American influence.

In the spring of 1909 the Department of State demanded of China that American bankers be granted entry into the Hukuang loans. This move grew out of the realization after the Russo-Japanese War that the commercial interests and political influence of the United States were in decline. Equality of commercial opportunity had no real meaning when the nations that gained contracts for the railroads dictated the purchase of goods. This simple hypothesis explaining the failure of the Americans to capture the China market satisfied Philander Knox, the new Secretary of State under William Howard Taft. It likewise provided a rationale for Willard Straight who, as a consul in Manchuria, had witnessed Japan's taking advantage of its military control to promote sales of Japanese goods. It was Straight, head of the newly created Division of Far Eastern Affairs, who pushed the Taft administration into demanding that a consortium of American bankers be admitted to participation in the loans for financing the proposed construction of a railway network in central China. The European governments and bankers gave reluctant consent to American participation, but a prolonged hassle over the percentage of the total loans to be granted the Americans resulted in a long delay. It was the Department

⁵⁹ The facts on the American China Development Company are derived from the material in the Department of State Archives. For a full account of the diplomatic aspects, see William R. Braisted, "The United States and the American China Development Company," *Far Eastern Quarterly*, XI (Feb. 1952), 147-65.

of State and not the bankers who insisted on an equal share for American capital.

In September 1909 came the Knox proposal for an international loan to China enabling it to buy the railways in Manchuria owned by Japan and Russia. When this plan collapsed, the United States proposed a currency loan that would enable China to develop Manchuria. Again Japan and Russia opposed the project and insisted on reservations to protect their own interests. The chief importance of both the railway project and the currency proposal lay in the fact that for the first time the United States government sought to promote exports to China by means of American investment.

Measured against these actualities the rhetoric concerning the China market ran so wild as to suggest that it was in the nature of a myth. Indeed, the gap between the rhetoric and the actualities attained dimensions of so great a scope as to suggest that the sheer joy of the discussion and not facts sufficed as a propellant.

* * * * *Note* * * * *

Beard on Foreign Policy

IN February 1941 Charles A. Beard appeared before the Foreign Relations Committee of the United States Senate to present a statement against the lend-lease bill (S. 275). Beard opposed the measure on the ground that its language was sweeping and imprecise, that it would involve an unconstitutional surrender of congressional powers to the executive, and that it would result in the pouring out of American wealth and possibly lives for the sake of unattainable objectives. Noting that the bill had been called a bill to keep the United States out of the war, he cited historical examples of "intentions utterly defeated, of consequences unforeseen . . . , of disasters un contemplated, [and] of capricious changes in our opinions and slogans, that should sober us all into humility. . . ." The "older I grow," he added, "the more I am convinced that the wisest amongst us knows little about the great history in which we are now acting." (US Congress, Senate, *Hearings*, Pt. 2, "A Bill Further to Promote the Defense of the United States, and for Other Purposes," 77 Cong., 1 sess. [Washington, D. C., 1941] 308.)

A visiting professor at the Johns Hopkins University at the time, Beard returned to Baltimore to answer in the columns of the Baltimore *Sun* a bitter attack upon his testimony by Arthur O. Lovejoy, professor of philosophy at that institution. That summer, in response to a letter in which an undergraduate history major at Hopkins had sought to account for the rise of Hitler and had urged American participation in the war, Beard wrote the letter that follows. The document is important not only because it gives a succinct summary of Beard's reasons for opposing American entry into the "shooting war" but also because of the further evidence it provides of the way in which Beard related history to questions of current policy.

Columbia University

STUART BRUCHEY

New Milford, August 23 [1941]

Dear Mr. Bruchey,

As to the facts which you bring into consideration in your thoughtful letter

there is nothing from which I can dissent. You take your stand, on solid historical grounds. [*sic*] and any decision in policy which you may make I should not venture to call "wrong." On the contrary I should respect you for making it—Hitlerism is equally hateful to us, I am sure. Whatever happens in Europe we are in for grave troubles and difficulties here. Will it be better for us and the world if we plunge into the "shooting" war? Given the state of the nation, the quality of our political leadership, and uncertainty as to *where* to begin shooting, I think the answer is negative but I make it under full consciousness that it is at best or worst merely my own intuitive judgment based on my own knowledge and experience. I find no way in life of avoiding the perils of decisions. Even in small matters there are hazards. In large matters they are more difficult and hazardous. Yet make them we must while we live.

With all good wishes.

Sincerely yours,

Charles A. Beard

* * * *Review Note* * * *

KRIEGSTAGEBUCH DES OBERKOMMANDOS DER WEHRMACHT (WEHRMACHTFÜHRUNGSSTAB), 1940-1945. Volume I, 1. AUGUST 1940-31. DEZEMBER 1941. Edited by *Hans-Adolf Jacobsen*. Volume II, 1. JANUAR 1942-31. DEZEMBER 1942. In two parts. Edited by *Andreas Hülgruber*. Volume III, 1. JANUAR 1943-31. DEZEMBER 1943. In two parts. Edited by *Walther Hubatsch*. (Frankfurt am Main: Bernard & Graefe Verlag für Wehrwesen. 1965; 1963; 1963. Pp. 232, 1285; xii, 780, 781-1464; xi, 730, 731-1661. DM 158; DM 168; DM 182.)

AMONG the German military records captured by the American armies at the end of World War II were various parts of the War Diary (*Kriegstagebuch*) of the Armed Forces Operations Staff (*Wehrmachtführungsstab*) of the High Command of the Armed Forces (OKW). These constitute a unique source. There is nothing like it on the German side for World War I, no daily record of the situation as it appeared at the *Oberste Heeresleitung*, and no institutional basis for anything comparable in the United States or Britain.

Professor Percy Ernst Schramm, recently retired at Göttingen, served as general editor of the series, bringing to his task a medievalist's respect for documents and his wartime experience as reservist major. In March 1943 he succeeded Helmuth Greiner as keeper of the *KTB/WFSt*. Schramm launched the series in 1961 with Volume IV (1944-45) which includes an account of how he and Greiner managed to frustrate destruction of the diary despite military orders. (See the excellent review by Chester V. Easum, *AHR*, LXVIII [Oct. 1962], 128.) There was wholesale destruction of documents in the Nazi *Götterdämmerung*; yet Schramm estimates that roughly three-fourths of what Greiner and he recorded survived.

What Schramm and his very competent team of former students become colleagues have issued is a composite work. Where the original *KTB* has disappeared, they have filled in with other documents so that we have continuity of coverage, a running view of the situation on all fronts from Hitler's headquarters, and a record of his decisions as supreme commander. The set is like a precious antique, tossed out when the house burned down, with only portions of the original saved but with the missing parts skillfully reconstructed by master craftsmen.

Each volume comprises, besides the texts for its period, a preface by the editor explaining its parts; a narrative history from the German point of view; a documentary appendix of related German military papers not previously published; various editorial appendixes—table of abbreviations, code names, organization of the German Army, of the higher ranks, chronology, person, and subject index. These have all been prepared with Teutonic thoroughness and most scrupulous regard for the best traditions of German scholarship. Of the original texts nothing has been changed, nothing omitted. The editing and narrative parts show a mastery of the German literature on World War II that has now grown to massive proportions. No institution with pretensions toward serious study of World War II can dispense with this magnificent source collection.

Volume I by Jacobsen, aside from a few stray entries from the lost *KTB*'s of 1939, is based on five principal elements: (1) The diary entries of the *WFS*, August 1, 1940–March 24, 1941 (pp. 1–367); (2) Memorandums by Greiner on the discussion of the situation, notes on the basis of which he composed the *KTB*. These cover the period August 8, 1940, to June 25, 1941, and are interlarded with the diary entries, each set off under the rubric *handschriftlich* (pp. 15–410); (3) The War Diary of the Operations Division of the Army General Staff, May 27–September 4, 1941 (pp. 411–88); (4) The daily reports of the Operations Division of the Army General Staff, June 22–December 6, 1941 (pp. 490–797); (5) Secret daily intelligence summaries regarding the situation, submitted by *Amt Ausland/Abwehr*, December 7–December 31, 1941 (pp. 797–873). Thus this volume presents considerable chronological overlapping.

No parts of the original *KTB/WFS* survive for the year 1942. The reconstruction in Hillgruber's volume is based on: (1) Greiner's handwritten notes for the period August 12–December 31, 1942; (2) The situation reports (*Lageberichte*) of the OKH (Operations Division, General Staff of the Army), January 1–February 6, and May 1–December 31; (3) Daily reports of the *Luftwaffe* Operations Staff, July 1–December 31; (4) Documentary material collected for the Historical Section of the OKW; (5) Excerpts from Halder's diary for the gap of February 7–March 31. Among Greiner's notes is a series of postwar commentaries (*Erläuterungen*) by General Warlimont, which often supply useful corrections or supplementary details.

Volume III presents the original *KTB/WFS* for the first, third, and fourth quarters of 1943 and a reconstruction for the period April 1–June 30 based largely on a manuscript prepared in 1950 by Warlimont for the US Army (P-049, "OKW Activities"). This series offers a view that will endure. Only minor modifications are likely to develop from naval documents not yet published.

Between war's end, when Schramm and Greiner saved the documentary materials from destruction, and their restitution to the Federal Republic which enabled Schramm to launch his project, they were in the custody of the US Army, Historical Division (Office of the Chief of Military History). Some parts were extensively used for the "History of the U.S. Army in World War II," both in the seven volumes on the European theater of operations and in the two volumes that so far have appeared on the Mediterranean theater. By courtesy of the OCMH Greiner's notes and memorandums were made available for editing certain volumes of the *Documents on German Foreign Policy* for 1940–1941. For a comprehensive listing of the German materials assembled by the Army Historical Division, see its "Guide to Foreign Military Studies, 1945–54, Catalog and Index" (1954) and "Supplement" (1959).

These German scholars have produced in short span a comprehensive source collection of basic importance for World War II. But much preliminary work had already been done in the Historical Division and its European command. Many questionnaires, commentaries, and narratives by German officers had been gathered and reproduced (without copyright), and these were at hand when Schramm launched his project. Warlimont's *Erläuterungen*, which are so nicely fitted in with the texts in the German publication, were begun under American auspices; the OCMH also deserves credit for its part in this publication.

Department of State

HOWARD M. SMYTH

* * * * *Reviews of Books* * * * *

General

SOVETSKAIA ISTORICHESKAIA ENTSIKLOPEDIIA [Soviet Historical Encyclopedia]. Volume I, AALTONEN-AIAYN; Volume II, BAAL-VASHINGTON; Volume III, VASHINGTON-VIACHKO; Volume IV, GAAGA-DVIN; Volume V, DVINSK-INDONEZIIA. *E. N. Zhukov*, General Editor. [Nauchnyi Soviet Izdatel'stvo "Sovetskaia Entsiklopediia," Otdelenie Istoricheskikh Nauk, Akademiia Nauk SSSR.] (Moscow: "Sovetskaia Entsiklopediia." 1961; 1962; 1963; 1963; 1964. 1024 cols.; 1022 cols.; 976 cols.; 1072 cols.; 960 cols.)

In its reach this encyclopedia may well surpass any other work of its kind devoted entirely to history, for when complete it will comprise 25,000 articles in twelve large volumes. Though it claims all of human history as its province, earlier periods receive less attention than modern and contemporary ones, and Russia and its peoples 38 per cent of the space as against 62 per cent for the rest of the world. For what purports to be a universal work the Russian emphasis seems exaggerated; on the other hand, the editors commendably have chosen to devote 40 per cent of the remaining pages to Africa, Asia, and Latin America. As the editorial board and the roster of contributors include many of the Soviet Union's leading historians, and as the volumes boast abundant illustrations, maps, and statistical tables, one might expect this encyclopedia to be an invaluable reference work.

Actually its value is rather limited owing to certain well-known predilections of Soviet historical science, whose attainments, the preface states, are reflected in the work. The numerous articles are designed to help orient the reader "to the development of the world-historical process" "whose basic content, as the Program of the Communist Party of the Soviet Union points out, is the transition from capitalism to socialism." "Bourgeois objectivity," it would seem, is systematically excluded, and yet some articles are in fact unexceptionable, while others exhibit varying degrees of tendentiousness along with varying quantities of reliable data. Nevertheless, the primary thrust of the work is such that those who are not Marxist-Leninists are apt to use the encyclopedia less as a general historical reference than as a handy guide to the current views of Soviet historiography on a vast range of topics. Students of Russian history, in particular, should find the work useful for this purpose as well as for the bibliographies provided.

The qualities of the work may be further elucidated by some illustrations. Very properly, Germany is allotted one hundred columns; but the Belorussian SSR receives thirty-two, and Buddhism only four. Dobroliubov gets space equal to Bismarck and more than George Washington. An obscure Russian revolutionist named I. I. Gaza is given only a few lines less than Gambetta, and many more

than Bukharin who, as an unperson, does not appear at all. Short articles on such European historians as Acton, Burckhardt, Halévy, and Diltthey give tolerably adequate accounts of their work, but are marred by the annoying practice of relegating each author—most historical figures are also similarly treated—to some overly simple sociopolitical category. Beard is the subject of an informative article; Herbert Aptheker gets only a few lines less than George Bancroft, and neither Henry Adams nor H. H. Bancroft appears at all. In general, European history and historians are dealt with more satisfactorily than American; the bibliography listed for George Washington includes five works by Marx, Engels, or Lenin, three Soviet volumes, and only two by American writers (Hughes and Thayer), the most recent published in 1930.

This encyclopedia affords glimpses of the potential of Soviet historiography as well as abundant evidence of the baleful influence of political direction on historical work.

University of California, San Diego

SAMUEL H. BARON

THE NATURE OF HISTORICAL THINKING. By *Robert Stover*. (Chapel Hill: University of North Carolina Press. 1967. Pp. xvii, 276. \$7.50.)

IN the ongoing debate among philosophers, historiographers, social scientists, and others that has become so prominent during the past two generations, Professor Stover presents a book whose effect will be, he hopes, to end the "sterile controversy." He strongly contends both with those who regard history as a science, to be comprehended purely in terms of "natural order intelligibility," and with those who see history as a discipline *sui generis*, to be pursued and understood only according to a mode of thought peculiar to itself. He ultimately concludes that "the nature of historical thinking" is not to be distinguished from the nature of thinking about human affairs in general, and that this, in fact, is pluralistic; that is, it is possible to think about the human past, present, and future both from a deterministic or natural order viewpoint and from a personally or socially "caring" view. In the latter case human will and decision come into play. This he calls the "living in the world" view. The two views are not mutually exclusive but alternates: the first aiming primarily at intelligibility, the second at living and acting in the world. They may, and usually do, intermingle in any person's thinking. Neither along one line of thinking nor the other does one come to any unitary conception of history, an outlook that the author emphatically rules out. In that regard he appears to come to conclusions that are essentially those of the secular liberal-rationalist, though they are qualified by a greater sense of limitation and variety than was customary in the heyday of that outlook during the Age of Enlightenment and the nineteenth century.

Most ordinary historical writing, and thinking, is carried on in such a way that it "dooms to futility all efforts to discover the essence of what historians ordinarily do." Historical thinking is mostly imprecise, vague, and fuzzy. Nonetheless, it has "profound philosophical import" for it "mirrors concretely the pluralism painstakingly discerned and articulated through critical reflection."

Stover hopes to persuade historians to think more precisely and to express

themselves more exactly in their writings. I fear, however, that a book written in the first instance for philosophers and couched in heavily philosophical terminology will find but a small audience among historians who, just because they are, as the author so frequently asserts, so close to everyday life and thought, find it necessary to express themselves in everyday language.

University of Toronto

RICHARD M. SAUNDERS

A HISTORY OF IDEAS ABOUT THE PROLONGATION OF LIFE: THE EVOLUTION OF PROLONGEVITY HYPOTHESES TO 1800. By *Gerald J. Gruman*. [Transactions of the American Philosophical Society, New Series, Volume LVI, Part 9.] (Philadelphia: the Society. 1966. Pp. 102. \$3.00.)

Dr. Gruman has examined legends and folklore, and philosophical, religious, and scientific texts from Sumeria to about 1800, for their bearing on man's efforts to increase his life span, to denote which he has coined the term "prolongevity." Apart from early myths of unusual longevity in past times, in other places, or through special substances (the antediluvian, Hyperborean, and Fountain of Youth themes), the author finds in Taoism the first systematic efforts based on "proto-science" to promote longevity as both possible and desirable. While the skeptic may interpret the transformation of the "adept" into an immortal hsien through "deliverance of the corpse" as more akin to life after death than to earthly immortality, other aspects, especially its physiological techniques, contributed to Chinese medicine, and they justify the emphasis on Taoism.

Chinese alchemy, closely tied to Taoism, is also considered, especially for its possible influence in directing Arabic and, by derivation, Latin alchemy toward prolongevity. This was especially strong in the work of Roger Bacon. Bacon, however, no longer thought in terms of immortality but of a limited though longer life span. He and later alchemists until the fifteenth century were increasingly pharmacologic in their outlook, thus transforming alchemy into medicinal chemistry. Gruman next examines hygienists like Luigi Cornaro, who sought to promote longevity through temperate living, and leads, finally, to the philosophes Franklin, Godwin, and Condorcet, who saw prolongevity as a future goal to be reached through the application of science and reason in improving the condition of man.

Gruman has selectively assembled strands from widely diversified systems of thought in an original way. Literature on the history of gerontology is rather sparse and oriented toward aging and the medical treatment of the aged; literature on the history of public health and demography is concerned with the increase of average life expectancy, a much different concept. The author has widely sampled original sources, mostly in translation, but of necessity he has relied heavily on secondary works. While the monograph seems at times somewhat disjointed and the analysis too brief to be always convincing, the theme is significant in relation to modern ideas about science and progress, and the work is interesting and suggestive.

National Library of Medicine

JOHN B. BLAKE

ANNALES DE DÉMOGRAPHIE HISTORIQUE, 1966 (ÉTUDES, CHRONIQUE, BIBLIOGRAPHIE, DOCUMENTS). Director, *P. Goubert*. Editor in chief, *J. Dupaquier*. [Société de Démographie Historique.] (Paris: Éditions Sirey. 1967. Pp. 440. 32 fr.)

This yearbook is the third of a series, the only periodical devoted exclusively to historical population problems (see *AHR*, LXXI [Apr. 1966], 897). Among the eight articles, the one by P. Galliano on infant mortality in the southern outskirts of Paris from 1774 to 1794 is unusually interesting, because it pays special attention to the boarding of infants with wet nurses, which was more common in France than in any other country. An official *Bureau des Nourrices* mediated the distribution of infants in the environs of Paris and in the provinces where they were sent in the first few days after birth, during transport often surviving on syrup instead of milk. The nurslings belonged to all classes, from noble and bourgeois mothers who disdained breast feeding their children to urban laborers and domestics whose work made it inconvenient to do so. Since the mortality of the *nourrissons* exceeded that of the native infants and prolonged suckling produced temporary sterility in the wet nurses, the author questions whether the custom accounts in part for the early retardation of French population increase. Another significant aspect is the lack of concern shown by many parents in placing their children: women in whose care several children had died did not lack new clients.

P. Riché devotes an essay to population in the early Middle Ages without adding anything to J. C. Russell's writings on the subject. P. Guillaume discusses the problems of demographic history pertaining to a provincial city in the nineteenth century. A. Armengaud shows that the great majority of French syndicalists, anarchists, and socialists received the Neo-Malthusian propaganda for family planning with indifference. K. Obermann compiled census data for the German Confederation and its member states in the first half of the nineteenth century. E. Vielrose computed figures on internal migrations in the area of the United States during the second half of the eighteenth century based on S. Blodget's estimates for 1753 and the censuses of 1790 and 1800. J. N. Biraben gives a comprehensive quantitative account of the peopling of French Canada beginning with prehistoric Indian settlements, continuing with French settlements since the sixteenth century, and finally surveying the population of French extraction and language in present-day Canada; he does not utilize any English-language research in French-Canadian natality and related problems. An important feature of this and the previous volumes is a 150-page bibliography; haphazardly collected, only partly annotated, arranged by countries, but not cross-indexed for demographic characteristics, it is a cumbersome but unique research tool.

Boston University

HERBERT MOLLER

CIVILISATION MATÉRIELLE ET CAPITALISME (XV^e-XVIII^e SIÈCLE).
Volume I. By *Fernand Braudel*. [Collection *Destins du Monde*.] (Paris: Librairie Armand Colin. 1967. Pp. 463. 75 fr.)

WHAT is perhaps most significant about this remarkable book is not that it is

done so well as that it needed doing at all. The importance of the material factor in civilization has long been postulated by historians, but it has generally meant little more to them than a crude economism; they have paid little attention to the actual objects of the past. Unlike their congeners, the archaeologists, they have seldom studied the specifically material culture except *en passant*, as a by-product of their primary investigations or their browsing. Now, for the first time, we have a systematic descriptive and analytic study of the material civilization of the world during the early modern epoch. Fernand Braudel has met this extraordinarily difficult and self-imposed task with a competence that would be amazing if we did not already know the characteristic combination of erudition, understanding, and expository skill of the author of *La Méditerranée et le monde méditerranéen à l'époque de Philippe II*. This book is actually only the first volume of a two-part work; the second, as the inclusive title indicates, will be devoted to the complementary subject of capitalism. The present volume, complete in itself, treats in turn the complex, puzzling, and fundamental questions of demography; the agronomic characteristics of the principal foodstuff in each of the great civilizations and its consequences for the structure of the society it feeds; the other food and drink, both common necessities and the luxuries of superfluity; habitations, clothing, and fashion; technology, especially the sources of energy, metallurgy, inventions, and transportation; money, and monetary instruments and systems; and, finally, the cities, as physical structures, ways of living, and "worlds in imbalance." In the course of a brief notice it is impossible to discuss at length any of the multitudinous problems of major importance that Braudel clarifies. His analyses and suggestions will no doubt be taken up by specialists and stimulate further work. Braudel does not claim to have final answers or even always the right questions, but he does distill the essence of the present knowledge, cut through the received notions, and suggest new ideas, and he indicates his own uncertainties even while doing so. His modesty regarding what can be done with numerical data is laudable in one so deeply devoted to structural history. Braudel's primary contribution is to make clearer than ever before the fundamental character of the early modern period as the time of transition, in Europe, between the old period of limited productivity in which Malthus' somber vision held true and a new world able, thanks to technological and economic break-through, to escape the doom of Sisyphus and to create prosperity for the many.

In itself, of course, this picture is not new, but it acquires from Braudel's pen new geographic scope, fullness of detail, and sharpness of outline; it covers all continents, not just Europe. The book's value is increased by a wealth of illustrations, always informative and often aesthetically delightful; they are invariably fresh, for Braudel has tapped seldom used collections in many countries and found new things even in places like the Bibliothèque Nationale. It is to be hoped that some enterprising publisher will bring out this valuable work in English and keep its cost below the quite excessive French level.

Rutgers University

HERBERT H. ROWEN

THE ENLIGHTENMENT: AN INTERPRETATION. THE RISE OF MODERN PAGANISM. By *Peter Gay*. (New York: Alfred A. Knopf. 1966. Pp. xviii, 555, xv. \$8.95.)

IN this work of admirable erudition and, at the same time, of extreme personality, Peter Gay exhibits an immense range of learning; he also starts an argument and places himself and his own values squarely in the center of it. Surely, the author's very combativeness, expressed as it is in clear, lively English, helps to account for his readable volume's having won a National Book Award. An indication of both its scholarly breadth and its personal candor is the bibliographical essay, 129 pages long and studded with many more first person singular pronouns than the genre customarily accommodates.

When Gay has completed his task, he tells us, there will be two volumes, together providing "a comprehensive interpretation of the Enlightenment." In the one at hand, he explores the dialectical background for the Enlightenment's own, positive program, which he intends later to treat as "The Pursuit of Modernity." This initial volume necessarily contains much on the thought of Greece and Rome in Book One, "The Appeal to Antiquity," and of the Middle Ages and the Renaissance in Book Two, "The Tension with Christianity." But it is the eighteenth century's confrontation of "Hebrews and Hellenes," of Christian and pagan ideals, that finally concerns him. As his subtitle announces, the Enlightenment is for him a triumph of neopaganism, and, as such, he welcomes it: "To the degree that this book can claim to move beyond historical analysis to a philosophical comprehension of the past, it decides between the Christian millennium, with its ideal of dependence, and the Enlightenment, with its ideal of autonomy, in favor of autonomy." The autonomy at issue is summed up in the concluding sentence about David Hume, who "makes plain that since God is silent, man is his own master: he must live in a disenchanted world, submit everything to criticism, and make his own way."

My own reaction to Gay's study is complicated by several considerations. On the one hand, I enjoy the richness of his knowledge, here displayed even more lavishly than in his *Voltaire's Politics* (1959) or *The Party of Humanity* (1964). On the other hand, I am left with three sets of misgivings, of which two are perhaps sufficiently idiosyncratic and even elusive to be dismissed as minor. The third, however, is fundamental.

As for the lesser objections, the first has to do with the tenor of debate. Gay is a learned and a careful scholar; he is also a strong and convinced protagonist. The result is a way of constructing his argument that appears to take full account of reservations, exceptions, and related difficulties, but in fact reduces very little the force of a quite extreme interpretation. Thus, a paragraph frequently opens with a generalization that is followed by several thoughtful qualifications and concessions; the paragraph then concludes with a restatement of the generalization, somewhat reshaped but seldom weakened. The result is that beneath the contours of an analysis softened by subtlety and learning, the hard framework of subjective assertion is essentially unchanged. I am not sure that this is a proper subject for complaint, for there is no subterfuge involved; Gay cheerfully takes his reader into the game. Yet a lawyer's skills, applied to the

historian's craft, inevitably make those of us who are less skilled a trifle uneasy.

My second quibble relates to levels of seriousness. Gay can be witty, like the philosophes themselves. He gives us, for example, the words of Lichtenberg in the 1770's, thanking God profusely "for letting him grow up to be an atheist." There are times, however, when the author's evaluation of eighteenth-century writers seems to me to underrate the significance of oblique, perhaps unrecognized, motivation and, on occasion, of pure mischief. No one has done more than Gay to demonstrate the value of reading carefully the men of the Enlightenment, giving them the chance to say what they meant. In that effort, however, I believe we can still use at least a little of Carl Becker's impishness, and considerably more of Frank Manuel's psychological insight.

Last, and most important, there remains the question whether "modern paganism" really will bear the full weight as a general interpretation of the Enlightenment. Gay's book convinces me, at least, that it is much more important than many earlier treatments of Christian humanism and deism have conceded. Yet the Enlightenment had other important characteristics as a movement: reliance on a particular *kind* of reasoning (analytical), a pragmatic view of benevolence and beneficence, and a reverence for science, none of which were intrinsically pagan, however unsettling they may have been for religious conservatives.

In this regard, the present book's structure is revealing. For Gay, the culminating figure and philosophical stance—as Kant and critical rationalism once were for Gay's master, Cassirer—turn out to be Hume and radical skepticism. Not even the old battler, Voltaire, goes far enough. Lessing gets full treatment as a syncretist and doubter, but Lessing cannot be made into a "pagan." Gibbon, of course, can; Rousseau, of course, cannot. And so it goes when we fall to trading cases across the counter of general interpretation.

Harvard University

FRANKLIN L. FORD

SLAVERY IN THE AMERICAS: A COMPARATIVE STUDY OF VIRGINIA AND CUBA. By *Herbert S. Klein*. (Chicago: University of Chicago Press. 1967. Pp. xi, 270. \$6.95.)

SOME years ago Frank Tannenbaum and Stanley Elkins developed the thesis that slavery in the southern United States had certain unique qualities that produced a peculiarly dehumanizing effect on its victims, whereas the more traditional form of slavery in Latin America protected the slave as a human being. Since Tannenbaum and Elkins have been criticized for confining their comparative analyses to formal legal structures, Herbert S. Klein examined the cases of Cuba and Virginia to determine "the social and economic dynamics of the New World slave systems." His conclusion is that the internal conditions of the two regimes differed as sharply as the legal codes that supported them and that the earlier studies of Tannenbaum and Elkins are thus confirmed.

In Cuba, Klein argues, the master was not the sole source of authority or the slave's only contact with the outside world. Standing between master and slave were two powerful institutions, the crown and the Catholic Church, both of which were motivated by noneconomic considerations to demand the recognition of the

slave's humanity. Moreover, Cuban slavery was an open system from which slaves escaped with relative ease. Free Negroes and mulattoes became an integral part of a society that emphasized class rather than caste, thus affording them considerable social mobility. In Virginia, according to Klein, the conditions of servitude were entirely regulated by the economic interests of the tobacco planters, for neither church nor state had the autonomous power to intervene in the slave's behalf. Slavery was a closed system from which few escaped, and those who did entered a hostile society based on a rigid caste system. "If anything stands out in sharpest contrast," Klein concludes, "it is Virginia's 'Legislation of Iron'—created as it was by 'pure' capitalism—and Cuba's slave regime, a product of historic institutions and ancient philosophies alien to the modern capitalist temperament."

Klein's monograph illustrates both the possibilities and the pitfalls of comparative history. He has found some valid and significant contrasts in the two slave systems. To give some examples, the Spanish slave code of 1789 was clearly more liberal than any of the Virginia codes; the Catholic Church, unlike Virginia's Protestant churches, actively worked to protect the marriage sacrament among slaves; the problem of runaways was more serious in Cuba than in Virginia; a higher proportion of Cuban slaves lived in urban centers and were engaged in nonagricultural occupations; and, finally, the more numerous individual acts of emancipation in Cuba produced by 1860 a free Negro and mulatto population that was proportionately three times as large as in Virginia.

Unfortunately the positive contribution of this monograph is obscured by its numerous and glaring weaknesses. Better copy editing might have remedied its awkward prose, misuse of words, and repetitiousness; but there are more serious problems: omissions of significant data, contradictions and inconsistencies, and, above all, inadequate research. Klein examined a substantial number of Cuban sources, but he relied upon a scattering of secondary works and public documents for Virginia. As a result, he knows little about the complexity of Virginia slavery, and less about its "social and economic dynamics." Klein has shirked the heavy burden imposed on those who attempt to write comparative history: the need to understand thoroughly *both* historical situations that are to be compared.

Klein's inadequate knowledge of Virginia slavery leads him into numerous errors and distortions. For example, he is mistaken when he claims that Virginia's code recognized the slave's humanity only to hold him responsible for crimes. By the nineteenth century it also contained various provisions against mistreatment; and, contrary to Klein, it did in fact permit state interference in the master-slave relationship. Klein cites an early Virginia law providing that the killing of a slave by his master was not a felony, and he asserts that this "well represents the ultimate direction in which the Virginia codes would run." He either does not know or deliberately ignores the fact that a new law, passed in 1788, defined the malicious killing of a slave as murder subject to the same penalty as the murder of a white man. Klein is also mistaken when he asserts that Virginia's evangelical churches lost interest in the conversion and religious instruction of the slave, and that the role of the slave in these churches "quickly declined to the same point as his role in the Anglican church and its successor."

Equally serious, Klein succumbs to a temptation in comparative history to exaggerate differences and ignore similarities. It is not difficult to find striking parallels in the legal codes and social dynamics of the two slave systems, yet Klein writes as if he were describing two totally different institutions. Slavery everywhere in the Americas had a dehumanizing effect on its victims, and Klein has by no means demonstrated that slavery as practiced in Virginia was more dehumanizing than in Cuba. It is safe to say that at no time was Virginia slavery as brutal as on nineteenth-century Cuban sugar plantations. But Klein seems to find brutality of little consequence, for he argues that a slave on a Virginia tobacco plantation "was in a far inferior position even to the toiling cane cutter of Cuba." Moreover, though Virginia's code never legalized slave marriages, it is quite likely that a higher proportion of Virginia slaves lived in family units than did Cuban slaves. In Cuba during the eighteenth and nineteenth centuries male slaves outnumbered females by nearly two to one, and on sugar plantations by four to one. This fact should be taken into account when one evaluates the role of the Spanish crown and the Catholic Church in protecting the slave as a human being.

What Klein does not seem to understand is that in both Cuba and Virginia, regardless of differences in legal codes, the degree to which slavery deprived the Negro of his humanity depended to a considerable extent on the nature of the individual master. He does not give enough attention to the day-to-day operations of the two slave regimes, and he is too much concerned with the same sort of comparative analysis of formal legal structures for which Tannenbaum and Elkins have been criticized. There is no evidence in this book that, compared to Virginia slaves, the Spanish crown and Catholic Church did in fact secure for Cuban slaves better food, clothing, and shelter, a lighter work load, less severe punishments, more protection for their persons, surer justice in the courts, or even a more secure family life. Indeed, in spite of his professed purpose of examining the social dynamics of slavery, Klein does not provide enough new data to advance the argument much beyond where Elkins left it in 1959.

The comparative study of slavery in the Americas, as it has been pursued to this point, makes it abundantly clear that new conceptualizations and methodological innovations, valuable though they may be, are poor substitutes for research.

University of California, Berkeley

KENNETH M. STAMPP

STANOVLENIE RUSSKO-AMERIKANSKIKH OTNOSHENII, 1775-1815
[The Establishment of Russian-American Relations, 1775-1815]. By N. N. Bolkhovitinov. [Akademiia Nauk SSSR, Institut Istorii.] (Moscow: Izdatel'stvo "Nauka." 1966. Pp. 638.)

THE stringent historical positivism that characterizes the best of recent Soviet scholarship appears again in N. N. Bolkhovitinov's detailed survey of early Russian-American relations. The book treats diplomatic, economic, and cultural contacts from 1775 to 1815, and though the author sets out to correct certain "erroneous impressions" perpetrated by "propagandistic" Western historians, polemic plays almost no part in the body of the work.

The author has used a wide range of Russian archival sources, contemporary

journals, memoirs, and correspondence, as well as a generous selection of micro-filmed American diplomatic documents, newspapers, and magazines. Although no broad interpretive theme integrates this study, Bolkhovitinov makes it plain that Russia's political relations were determined by *raison d'état* rather than theoretical or ideological commitment, and this characteristic, which is apparent in Russia's treatment of the colonies' revolt against England, may be extended into the era of the French Revolution. America was not a central issue for Russian diplomacy in the period covered, but a pattern of regular political contact was established, and a favorable atmosphere developed in which to discuss commercial questions, Latin America, and colonial competition in the Pacific Northwest. Regular trade expanded swiftly in the postindependence period, and Bolkhovitinov adds much to the limited store of data previously compiled on this point. On the cultural level the author describes the information available to educated Russians concerning the colonies and the War for Independence. The *Moskovskie vedomosti*, published by N. I. Novikov, reported the American war, and in special supplements presented biographical sketches of American leaders and other background material. Novikov's journal was more sympathetic to the colonists than its St. Petersburg counterpart, but both provided a substantial fund of information. Unfortunately, apart from a lengthy discussion of Alexander Radishchev's views, Bolkhovitinov does not pursue the reaction to the American experience or discuss its possible significance for Russian political ideas. The same deficiency occurs in his treatment of scientific relations, which becomes little more than a catalogue of points of contact. These reservations aside, this book is a major work of descriptive scholarship and deserves serious attention from historians both of Russia and America.

Temple University

RODERICK E. MCGREW

PIONEERS EAST: THE EARLY AMERICAN EXPERIENCE IN THE MIDDLE EAST. By *David H. Finnie*. [Harvard Middle Eastern Studies, Number 13.] (Cambridge, Mass.: Harvard University Press. 1967. Pp. x, 333. \$7.50.)

WHILE Mr. Finnie indicates that, strictly speaking, he is not writing a history of American relations with the Middle East, he has certainly filled in gaps in our knowledge, and he points toward an enduring theme in the American interest. "What is new," he suggests, "about America in the Middle East is a sense of strategic and political concern and responsibility, inevitably dominated by the subterranean treasures of the world sedimentary basin." In this new book, the author of *Desert Enterprise* (1958) has told of the early American experience from the end of the eighteenth century to about 1850. Yankee traders are said to have called at Iskenderun as early as 1676, while the Mocha trade dates from 1805. American missionary-educators began the work of soul saving and education in 1819 and laid the foundations of an enduring legacy in that area. Woven around the travels of John Lloyd Stevens, who visited the Middle East during 1835-1836, here is the story of the travelers, the traders, the missionaries, the explorers, and the diplomats who lived and worked in the Ottoman Empire

and Persia. As Finnie well notes, to bring these pioneers back to life is more than a sentimental pastime or journey; it is, indeed, "an exercise in the rehabilitation of a significant aspect of our national heritage."

The author has given a fascinating account of the American consulate in Smyrna and the work of David Offley and of the negotiation of the first American-Ottoman treaty of May 7, 1830, in which special attention is called to the labors of Henry Eckford and Foster Rhodes in reconstructing the Ottoman navy after the "most untoward incident" at Navarino (1827), as part of the inducement to secure the Sublime Porte's ratification. He calls attention to the inauguration in 1826 of the Department of State's language program in Arabic and Turkish in the consulates in Tunis, Tripoli, Algiers, and Tangier and notes that John P. Brown of Chillicothe, Ohio, was probably the first national official of any country to become dragoman in his legation, anticipating both the British and the French at Constantinople. Readers will be interested in the story of the US Mediterranean Squadron, beginning in 1815, as a precursor of the Sixth US Fleet and, if they are not familiar with his own account, will relish the pages given to Lt. William Francis Lynch's exploration of the Jordan Valley in 1838. The story of the early American missionary-educators is, of course, central to the theme, in view of the enduring character of their legacy in the area and the basic interest that they established.

This is a book both for the general reader and for the scholar and specialist concerned with American policy in the Middle East, and it should stimulate others to probe into the field. It is interestingly written and well documented and illustrated. The bibliography lists all the old books and memoirs that must now be read again by all those who have an interest in American relations with the Middle East, whether then or now.

American University

HARRY N. HOWARD

L'EUROPE ET L'AMÉRIQUE À L'ÉPOQUE NAPOLEONIENNE (1800-1815). By *Jacques Godechot*. [Nouvelle Clio: L'histoire et ses problèmes, Number 37.] (Paris: Presses Universitaires de France. 1967. Pp. 365. 22 fr.)

JULES-PAUL TARDIVEL, LA FRANCE ET LES ÉTATS-UNIS, 1851-1905. By *Pierre Savard*. [Les cahiers de l'Institut d'Histoire, Number 8.] (Quebec: Presses de l'Université Laval. 1967. Pp. xxxvii, 499. \$10.00.)

PROFESSOR Godechot's survey of European and American history during the Napoleonic era has many merits. It is a superbly written and quite comprehensive book. Though fairly brief, it pays considerable attention to economic questions, as well as to military, political, and institutional developments. The author has, moreover, succeeded in presenting meaningful evaluations of Napoleon and his policies.

Driven by unlimited personal ambition, Napoleon demonstrated in the end that Europe could not be unified without the voluntary cooperation of the majority of its inhabitants who became increasingly conscious of their national existence. Nevertheless, the author shows that the impact of the First Empire on the history of modern Europe was profound, especially as far as the political,

social, and administrative conditions were concerned. Godechot concludes his book with an excellent historiographical essay in which he presents the changing interpretations of Napoleon's authoritarian regime, including Georges Lefebvre's brilliant analysis.

Pierre Savard's thoroughly researched and documented book elaborates the views of Jules-Paul Tardivel, an American citizen who became one of Canada's most ultramontane publicists in the second half of the nineteenth century. The owner and director of *La Vérité*, who pleaded so ardently for the preservation of French civilization in Canada and in defense of conservative Catholicism, merits renewed attention primarily because of the fate of his counterrevolutionary notions.

As the author correctly observes, Tardivel used and abused abstractions without defining such terms as liberalism, Modernism, Gallicanism, or Americanism. He looked upon liberal approaches in the Church as well as in society at large as the evil that was bringing about a "general moral decadence." According to him, only the religion of Christ could solve the social question. He opposed godless public schools, labor unions, modern literature, and the theater. Above all, he attributed the decline of civilization to the influence of Freemasons and Jews. In Tardivel's world, Paris was "a modern Babylon" and the United States "a vast Sodom." Although he recognized a certain dualism in both countries, he concluded that neither the Third Republic nor the materialistically oriented United States had much to offer to French-Canadians. He warned them particularly to be on guard against America's imperialism and the evil consequences of its false prosperity.

This study contains many details of great interest to students of Canadian and Church history. I regret that the author's focus is rather limited. A critical analysis of Tardivel's views in the light of broader historical perspectives would have further enhanced the genuine contribution of this study.

Rutgers University

HENRY BLUMENTHAL

THE MILITARY ATTACHÉ. By *Alfred Vagts*. (Princeton, N. J.: Princeton University Press. 1967. Pp. xiv, 408. \$10.00.)

MILITARY attachés, like intelligence agents, perform their duties under a cloak of secrecy, and they are better known, as Alfred Vagts wryly notes, by their indiscretions and failures than by their achievements. In a sense, therefore, writing a book about the military attaché presents much the same kind of problems as writing one about the CIA, or any other intelligence agency. The records of such agencies are virtually impossible to obtain, and their activities have to be reconstructed from memoirs and other unofficial sources whose reliability is often difficult to determine. Their organization, functions, regular day-to-day operations, methods of obtaining information, and relations with other government agencies are concealed. All that is revealed to the public is the unusual and the irregular, the record of failures and the unsubstantiated charges.

Despite these and other difficulties inherent in the nature of the subject, Professor Vagts has been able to assemble an imposing body of material on the military attaché that provides both a chronological history of the institution and

an analysis of the various roles of the attaché. Securing information on the armed forces of the country to which he was assigned has always been one of the duties of a country's emissary abroad, but the military attaché, as a formal and recognized element of diplomatic representation, is of comparatively recent origin, dating from the early nineteenth century. In 1806, Napoleon appointed a captain as second secretary of the Vienna embassy, and in 1810 Austrian Field Marshal Radetzky issued detailed instructions for officers attached to legations abroad. The term attaché did not come into English usage before 1835, and the institution was not firmly established until the middle of the century.

One of the most troublesome aspects of the attaché system was the relationship of the officer assigned to the head of the legation, who represented the Foreign Office and was usually a civilian. As early as 1809, when Schwarzenberg was appointed ambassador to Paris, it was understood that the military member of his staff was under his control, and it was even stipulated that he would draw his pay from the civilian rather than the army pay office. Radetzky's instructions of 1810 also made the military responsible to the ambassador. But this problem continued to plague the various embassies down to World War I. Military attachés tended to be professionals of high rank and from socially prominent and aristocratic families, with excellent connections at home and abroad. As a result, many did not feel constrained to defer to their civilian supervisor on political matters or to refrain from communicating directly with their military supervisors at home. Part military, part diplomat, and part espionage agent, with divided loyalties and responsibilities, the military attaché always presented, and may still present, an anomaly within the diplomatic service.

Dartmouth College

LOUIS MORTON

THE SECOND WORLD WAR: A MILITARY HISTORY. FROM MUNICH TO HIROSHIMA—IN ONE VOLUME. By *Basil Collier*. (New York: William Morrow and Company. 1967. Pp. xiii, 640. \$8.95.)

THE principal problems in writing a summary account of such a major event as World War II are selection, balance, and emphasis. In this work, Mr. Collier, a respected British military historian, stresses ground fighting in Europe and North Africa. He begins with a prelude of events from 1918 to 1934, including a few pages on the Far East. The next two chapters deal with the breakdown in Europe from 1931 to 1939. The heart of the book, occupying the next seventeen chapters with the exception of one on the war in the Pacific, centers on the Russian fronts, Western Europe, and North Africa. The writer goes into considerable detail on some battles and campaigns and flashes past others with astonishing speed. For example, he devotes much attention to the campaigns in eastern North Africa and takes care of the landings in Morocco and Algiers and the race toward Tunis in three or four pages. He concludes his volume with two chapters on the conquest of Japan.

Collier's organization for recounting the war in Europe and North Africa makes good sense, and the book is well written. He combines the chronological approach with the topical by taking a definite period of time, such as 1939-1940,

and dealing separately with the different areas of combat. He then moves to the next segment of time and repeats the process.

In a summary account such as this, it is the prerogative and responsibility of the author to select that portion of the mass of events that he wishes to present. I feel, as I have indicated, that the war in the Far East is underplayed in proportion to its significance. The most serious flaw in the history of the war in Europe, in my opinion, is the almost complete disregard of the war in the air except as related to ground warfare. In general, Collier is reserved in his judgments. It is quite clear that he regards Montgomery more highly as a military leader than he does Eisenhower.

The bibliography is useful. I believe, however, that both the bibliography and the text could have been improved by more extensive use of the United States official military histories. The author cites Morison and a few books in the army's military series, but ignores other works in this series that greatly illuminate the conduct of the war.

University of California, Santa Barbara

A. RUSSELL BUCHANAN

HISTOIRE POLITIQUE DE LA BOMBE ATOMIQUE. By *Claude Delmas*.
(Paris: Éditions Albin Michel. 1967. Pp. 397. 19.20 fr.)

CLAUDE Delmas, French journalist and member of the International Secretariat of NATO since 1957, has written nearly a dozen books dealing with recent and contemporary international politics. This, his latest work, is a comprehensive and rather well-balanced summary of the political history of the atom bomb. The book is organized chronologically and begins with the destruction of Hiroshima and Nagasaki and ends with the fifth experimental nuclear explosion realized by the Chinese on December 28, 1966. In between are a dozen chapters centering around the initial reactions to the bomb, the early efforts at a system of international control, the impact of Russia's achievement of the bomb on policies and strategies across the world, and the achievement in the mid-1950's of an uneasy equilibrium grounded in fear of the bomb. There are interesting and provocative chapters on the Suez crisis, the Cuban crisis, De Gaulle's efforts to make France a nuclear power, and the Sino-Soviet difference and Chinese efforts to develop the bomb. The book ends with a brief attempt to set the bomb in the perspective of history. Delmas insists that the bomb has opened a new era in the life of the world, and has exerted a far greater influence on diplomacy than any unutilized weapon in history. He sees the regime of "pacific co-existence" as largely a product of the bomb, and argues that peace today rests primarily on fear of nuclear arms.

Though the book has no index and only a short bibliography, it has many valuable footnotes and is liberally sprinkled with quotations from key documents. All in all, it is a thoughtful and competent survey of the political and diplomatic history of the atom bomb, and historian, statesman, and interested layman should find it worthwhile.

University of North Carolina

CARL H. PEGG

Ancient

INSTITUTIONS DE L'ANTIQUITÉ. By *Jean Gaudemet*. (Paris: Sirey. 1967. Pp. xix, 909, 8 maps. 100 fr.)

THIS handbook is divided into three parts, called *Livres*. The first of these surveys the ancient Near East with chapters on Mesopotamia, Egypt, the Hittites, and Israel. *Livre II* deals with Greece from the Bronze Age through the Hellenistic period, and *Livre III* is on Rome. The author's conception of his task is revealed in the last sentence of *Livre II*: "Ainsi cette étude préliminaire de l'Orient et de la Grèce a-t-elle préparé à aborder avec des vues plus larges, celle, qui demeure fondamentale, du monde romain."

The treatment of Greek history has a few slips; for example, Pericles is said to have governed Athens from 444 to 429 "sauf pendant le bref intermède du retour de Cimon à Athènes." The treatment of Athenian legal procedure is brief, and there is no reference to J. H. Lipsius: *Das attische Recht und Rechtsverfahren* (1905-15).

Livre III is divided into three sections called *Titres*. The first includes accounts of the monarchy, of the political institutions of the Republic, and of its treatment of conquered territory. *Titre II*, the longest of the three, covers the period from the middle of the second century B.C. to A.D. 284. *Titre III* deals with the late Empire.

The work will be useful for reference, especially on the legal structure of Roman society; for example, the treatment of the *constitutio Antoniniana* is a singularly lucid introduction to the problem. Some reservations may be felt about its account of the politics of the late Republic; the role of *clientela* and *amicitia* in the struggles of that period is consigned to a footnote near the end of the relevant chapter.

Each of the three *Titres* concludes with a chapter on the sources and sanction of law, and these are perhaps the most valuable part of the book. They deal with the *legis actiones*, the formulary procedure, and the procedure *extra ordinem*. Clarity is achieved within a short space, as when the reader is warned against exaggerating the conventional character of the *formula*.

References to modern literature in the footnotes are plentiful and will help specialists in many of the fields surveyed. Occasionally the relation between reference and text is not wholly clear. The most striking example occurs on page 376, where references given on the Italic Social War of 90-89 include G. L. Cawkwell, "Notes on the Social War" (*Classica et Mediaevalia*, XXIII [no. 1-2, 1962], 34-49). Cawkwell's article is about the Greek Social War of 357-354.

University of California, Berkeley

RAPHAEL SEALEY

EARLY CHRISTIAN AND BYZANTINE POLITICAL PHILOSOPHY: ORIGINS AND BACKGROUND. In two volumes. By *Francis Dvornik*. [Dumbarton Oaks Studies, Number 9.] (Washington, D. C.: Dumbarton Oaks Center for Byzantine Studies, Trustees for Harvard University. 1966. Pp. xvi, 452; x, 453-975. \$20.00 the set.)

A SUBJECT of historical scholarship that has witnessed significant and rapid

progress during the last decades is the Byzantine theory of "Emperor" and "Empire." Scholars who made particularly important contributions to this field include Dölger, Grabar, Ostrogorsky, and Treitingner. While several of them touched, in one way or another, on the origin and background of Byzantine political ideas, this subject received no systematic treatment. This void has now been filled by this monumental work of F. Dvornik. Its sweep is enormous as it begins with Pharaonic Egypt and reaches down to the period of Justinian. Yet throughout this book, as was to be expected from one of the world's foremost historians of the Byzantine Empire, the discussion of political thought in earlier civilizations is directed toward the explanation of the origins of early Christian and Byzantine political ideas. This focus on kingship gives Dvornik's book its theme and its unity.

The first three chapters are concerned with the ancient Near East. They discuss Egyptian notions about the ruler as a human being divinized by the gods, as well as Sumerian ideas about divine rulers and Babylonian notions about kings being the adopted sons of gods. In contrast, the early Iranians did not regard their political leaders as divine beings, but Dvornik discusses in considerable detail the Iranian concept of *hvarena*, "the awful royal glory," an apparition of light surrounding the Iranian kings and, especially in the Zoroastrian view, a messianic restorer of the world, Saoshyant. The next two chapters take up Greek political theories and realities from Mycenaean days to the end of the Hellenistic period and show how, after Alexander's conquests, Oriental, notably Persian, political ideas fused with Greek notions. In these chapters the author lavishes special care on a discussion of fragments of Hellenistic treatises on kingship first investigated by E. R. Goodenough and related to Byzantine political theory by N. H. Baynes. In these texts Dvornik discovers a strong Oriental influence, especially from Persia, as exhibited in the notion of the king as the animate law, and this influence emerges as an important link connecting the ancient Orient with Byzantium. Two further chapters deal with Jewish and Christian views on kingship and on messianism, both in orthodox groups and in sectarian circles such as the Qumran community. Here, too, Dvornik stresses the influence of Oriental and Hellenistic concepts as well as the reactions of Jews and Christians to such influences and investigates in detail the novel features of Jesus' teaching about the kingdom of God. He then describes the Hellenization of Roman political theory culminating in Diocletian's reforms and Christian reactions to this "Roman Hellenism." All the features previously studied are then shown to converge upon what Dvornik calls "Christian Hellenism," that is, the political views underlying the Christian polity envisaged by Constantine the Great and given its theoretical foundation by Eusebius of Caesarea. The last two chapters describe the struggle of Christian Hellenism against its Christian and pagan opponents and the gradual definition of the proper relationship between imperial and sacerdotal functions in the course of the doctrinal controversies of the fourth and fifth centuries. Here a particularly original section deals with the controversial problems of the *Vita Constantini* attributed by tradition to Eusebius and with Constantine's alleged remark that God had appointed him bishop for all "outside the Church." Dvornik then shows how the processes mentioned—the conflict of Christian Hellenism with

its critics and the competition of *sacerdotium* and *imperium*—culminated in the legislation of Justinian. Here the emperor was declared the animate law, and his functions were said to include a concern for orthodoxy and for the saintliness of the priesthood. St. Augustine emerges in Dvornik's work only at the very end of the last chapter, and his political views are sketched only in the barest outline. The reason for this treatment, surprising at first sight, is that Dvornik sees in Augustine less an exponent of ancient political thought than the inspiration for the new medieval politics erected in the West by the barbarian invaders.

The above summary provides a very inadequate idea of the wealth of information contained in this important book and the stupendous mastery of source materials in what for most historians are distinct fields of study. It deals primarily with the history of political ideas, but demonstrates a thorough awareness of the political and social realities underlying the development of thought. It includes translations of the principal texts from Pharaonic to Byzantine times and thus provides, among other things, a kind of anthology of ancient political thought. The author adheres to a rectilinear and providential theory of history, especially of the history of ideas, according to which the worthwhile elements in Oriental and Hellenistic thought were incorporated into Jewish and Roman political thought, were then refined and spiritualized by Christianity and thus handed down to the Byzantine Empire. While, as indicated, this view gives unity to Dvornik's work, each chapter can stand by itself and be read with profit by persons who do not subscribe to the author's historical outlook, but want information on topics such as the Assyrian views on kingship or on Hellenistic ruler cults. The book concludes with a bibliography of almost one hundred pages that covers the entire range of ancient political thought down to Justinian. Two titles missing in this bibliography are James Pritchard's *Ancient Near Eastern Texts Relating to the Old Testament* and Ernest Barker's *Social and Political Thought in Byzantium*. The first two parts of the latter work, beginning with the accession of Justinian, offer much information on material now treated more fully in Dvornik's great new book.

University of Michigan

PAUL J. ALEXANDER

HISTORY AND CHRONOLOGY OF THE EIGHTEENTH DYNASTY OF EGYPT: SEVEN STUDIES. By *Donald B. Redford*. [Near and Middle East Series, Number 3.] ([Toronto:] University of Toronto Press. 1967. Pp. xii, 235. \$8.50.)

THE history of ancient Egypt is a mosaic with most of the pieces missing. The historian tries to reconstruct the picture, filling in the empty spaces on a moderately subjective basis. From the Eighteenth Dynasty (about 1550 to 1300 B.C.) the material is relatively abundant, and, paradoxically, this makes the disagreement among scholars sharper because there are more bases for argument. Into this arena moves a young scholar from Canada, who tries to apply rules of logic. He is chiefly concerned with two factors: the narrowing of the time field within which certain careers fell, and the working relations between individual Pharaohs. Working partnerships—coregencies—and family feuds are a large part of the story.

The author's admirable command of the material and relentless reasoning tighten up the probabilities in a fascinating and controversial period of human history. If his tone is sometimes gratingly magisterial, his argument is still commanding. He is less successful when he descends to speculation, as in the suggestion that the matriarchal tendencies of the dynasty had a Nubian origin. Sometimes he is carried away by his own reasoning, as when he emends a text to produce a different king's name and then treats the change as established fact.

Two important studies are the nature of the reign of Hatshepsut, a brilliant queen who ruled as a king, and the relationships of the heretic Pharaoh Akhenaten with his royal father and royal son-in-law. I have treated both of these themes differently from the author, and I am happy to confess that the author's arguments are most impressive. The argument that Akhenaten had a coregency with his father, Amenhotep III, is almost laid to rest. If only some stubborn souls were not still convinced that young King Tutankhamen was the son of Amenhotep III, so that the time between Amenhotep III's death and the accession of Tutankhamen has to be telescoped by a coregency to keep the young king young!

The author has made a positive contribution, which historians will increasingly have to acknowledge. The writing is essentially for professionals, rather than for an armchair public. For a Ph.D. dissertation, it is a brilliant achievement.

University of Chicago

JOHN A. WILSON

THE ETRUSCAN CITIES AND ROME. By *H. H. Scullard*. [Aspects of Greek and Roman Life.] (Ithaca, N. Y.: Cornell University Press. 1967. Pp. 320. \$8.50.)

THIS generously illustrated book is not a history of early Rome. It keeps the focus upon the Etruscan cities, interesting in themselves as well as for their contribution to the development of Rome and thereby to Western civilization. It is in a sense a sequel to the time-honored Dennis, *Cities and Cemeteries of Etruria*, but it also takes into account the accumulated archaeological evidence. Preceding the treatment of individual cities is a chapter on the land and the geographical factors affecting their development. On the long-debated question of the origin of the Etruscans, the author leaves open the final decision between the "Orientalists" and the "autochthonists." But sometimes, as at Tarquinii, he points out the continuous development from Villanovan settlement to Etruscan city, in so far agreeing with Pallottino's view that, whatever mingling of races was involved, the culture we know as Etruscan, culminating in the seventh to the sixth century B.C., was developed within Etruria itself and not carried by immigration from the eastern Mediterranean.

In sketches of the individual cities the author proceeds by regions: first the southern cities which were the heart of Etruscan handicrafts and art, then the metal-working northern cities, and finally the outlying Etruscanized areas of Latium, Campania, and the transpadane region.

A description of each site is enhanced by a plan and by photographs of the major remains of buildings, finds, and works of art. Descriptions are correlated with available testimony of ancient writers. Thus the book is both a "travel

book" and a history of the individual cities. In general, the testimony of ancient authors is accepted as authoritative and is supplemented rather than displaced by inferences from the finds.

Recognizing that evidence is lacking at many points, the author tentatively reconstructs the history and status of each city within the broad lines marked out by literary and archaeological evidence.

The book's contribution lies in its synthesis of evidence from various sources and in the reconstruction of the development, status, and history of the individual cities. The reconstruction is often bold, but the reader is kept informed what the evidence actually is and where it is supplemented by speculation. Such a reconstruction leaves a wide margin of possible error, but it is compelling and, in my opinion, essentially sound. The book is good preparatory reading for a pilgrimage to the cities themselves.

Vassar College

INEZ SCOTT RYBERG

WELTGESCHICHTE DES MITTELMEERRAUMES: VON PHILIPP II. VON MAKEDONIEN BIS MUHAMMED. By *Ernst Kornemann*. Edited by *Hermann Bengtson*. [Beck'sche Sonderausgaben.] (Munich: Verlag C. H. Beck. 1967. Pp. xvi, 1069, 11 maps. DM 35.)

KORNEMANN's synoptic history of the Mediterranean regions originally appeared in two volumes, the first in 1948 and the second in 1949. (It was not reviewed here.) Kornemann himself had died in 1946, and it was left to Hermann Bengtson to see the work through the press and to augment the footnotes with references to more recent publications. The two volumes have now been re-issued as one.

The present publication is nothing more than a slightly corrected version of the original. Changes consist largely in the elimination of plates and many of the maps, as well as the correction of simple factual errors noticed by reviewers (for example, Claudius on page 439 of the old Volume II has been replaced by Nero on page 933 of the single volume). Kornemann's own preface, dated 1946, has been removed, which is a great pity, since it explains the genesis of the book: "Dieses Werk ist ein Kriegskind . . . Gegenwartsnahe, nicht gegenwartsverfälscht muss eine neue Geschichte des Altertums sein."

The most astonishing feature of the present reissue is its documentation. Nothing has been done to the footnotes since Bengtson revised them in the 1940's. Ancient history has not stood still since that time, and the failure to add new bibliographical items is culpable. Even more culpable, however, is the failure to remove remarks like "noch ungedruckt" from references to works published over fifteen years ago.

Was the *Weltgeschichte* really worth republishing anyway, just as a period piece? It certainly cannot be claimed as a classic. Kornemann's geopolitical approach, together with his emphasis on great men of his own selection, is not very palatable. *Weltgeschichte* generally has had its day. Kornemann's interest, inherited from Von Gutschmid, in the Iranian influence on European civilization can indeed be instructive, but not when larded, as it is here, with abstractions. Reviewing the original publication of this work in the *Journal of Hellenic*

Studies, LXXII (1952), 142, Norman Baynes wrote, "A reviewer cannot but ask himself whether it was really a service to the memory of Kornemann to publish this book in its present form. It is not easy to stifle a doubt." With a reissue, it is impossible.

Harvard University

G. W. BOWERSOCK

BRITANNIA: A HISTORY OF ROMAN BRITAIN. By *Sheppard Frere*. [History of the Provinces of the Roman Empire.] (Cambridge, Mass.: Harvard University Press. 1967. Pp. xiv, 432, 32 plates. \$15.00.)

IN the last generation Roman Britain was subjected to intensive study, especially by British archaeologists. Old sites have been developed, and new ones discovered; in this, aerial photography has proved its value and been fully exploited. Thus, the time has come to write a comprehensive history of the island to the end of the Roman occupation, and this is precisely what Professor Frere has done. The first two chapters describe the Iron Age in Britain before the coming of the Romans. Chapters III-IX begin with Julius Caesar's invasions and take us to the end of the third century. Frere now interrupts his narrative in order to discuss the administration of Roman Britain, the Roman army in Britain, the towns, the countryside, trade and industry, and the Romanization of Britain. The last two chapters resume the story chronologically and close when the official connection of Britain with the Roman Empire ended in 410. A list of abbreviations, a good select bibliography arranged chapter by chapter, and a simple index complete the book. Little has escaped Frere's critical eye. He has drawn on reports of excavations, the finds (in particular, the coins and inscriptions), and the literary sources and has put together a story of absorbing interest and impressive detail. We meet not only the British leaders but also the Roman commanders and officials. Frequently events in Britain cannot be separated from revolts elsewhere in the Empire and from the increasingly common struggles for the imperial throne. Frere does not avoid problems; he commands the scholarly literature, and he does not hesitate to advance solutions.

The plates, many of them illustrating clearly the evidence revealed by aerial photographs, are excellently reproduced. The well-drawn maps are relevant to the text. The writing is lively, and the editing has been done efficiently. This is a superb book, which will surely remain standard for years to come.

University of British Columbia

MALCOLM F. MCGREGOR

DIE FREUNDSCHAFTSLEHRE DES PANAITIOS: NACH EINER ANALYSE VON CICEROS 'LAELIUS DE AMICITIA.' By *Fritz-Arthur Steinmetz*. [Palingenesia: Monographien und Texte zur klassischen Altertumswissenschaft, Number 3.] (Wiesbaden: Franz Steiner Verlag GmbH. 1967. Pp. xii, 225. DM 28.)

RECONSTRUCTION of monuments from fragmentary remains and of events from incomplete accounts is a task familiar to every historian. In its simplest form it is a satisfying job: to see every piece fit into place, or to know exactly what is missing, or to distinguish the areas which remain doubtful from those which are

clear. Nothing is more ambiguous, on the other hand, less capable of conclusive proof, and less generally convincing than the attempt to reconstruct and define a literary or philosophical work from writings which derive (or are thought to derive) from it. Dr. Steinmetz has done a thorough and workmanlike job in seeking to show that the only direct source of Cicero's *De Amicitia* was a discussion of friendship by Panaetius and, at the same time, to describe and place this original work. If at the end one cannot claim to have seen a word of Panaetius' discussion and has been given no ocular evidence that it was Cicero's source, or even that it existed, yet one has been led through an illuminating examination of Cicero's essay, of the background of its ideas, and of their relationship to what is known or reasonably inferred about the doctrines and attitudes of Panaetius.

A common drawback of the quest for sources is treatment of the work in hand as a kind of corpse to dissect, uninteresting except for information to be extracted from it. Although always aware of his many German predecessors, Steinmetz is fairly successful in dealing with the *De Amicitia* as an independent creative work, with its form (derived from Heraclides Ponticus), its date (September–October 44 B.C.), its occasion (a preliminary to the *De Officiis*), and its plan (essentially following Panaetius, but with one major insertion, §§ 36–44, based on Cicero's contretemps with C. Matius). He sketches a Panaetius who had abandoned the rigor of his Stoic school and adopted much from Aristotle and Theophrastus: thus the distinct Peripatetic traits in Cicero. The discussion of friendship in the second book of his *Peri tou kathekontos* was part of his general theory of human society and conduct.

This is a well-produced and well-indexed book.

Harvard University

ZEPH STEWART

ROMAN IMPERIALISM IN THE LATE REPUBLIC. By *E. Badian*. [Communications of the University of South Africa, Series B, Number 26.] (Pretoria: [the University;] distrib. by William Blackwells, Oxford, Eng. 1967. Pp. 83.)

THIS slender volume comprises six lectures delivered at the University of South Africa in July 1965. The primary thrust of the material is to demonstrate the real nature of imperialism in the late Roman Republic, and, in the process, to lay to rest some well-worn myths. The work is to a degree preliminary to a forthcoming book of the author on "Roman Provincial Administration in the Republic," which will be well worth the reading.

Badian emphasizes that Roman imperialism from the middle Republic through the first decade or so of the first century B.C. was limited: "hegemonial" rather than "annexionist," at least in the East. In the West, against more barbarous peoples, the Roman policy was harsher.

A chief point is that Roman imperialism was not primarily motivated by economics until quite late; nor was it instigated by the moneyed, equestrian class. Even after the Social War brought large numbers of moneyed Italians into the equestrian class as citizens, economic penetration outside the bounds of empire seems to have developed little, though there was a more thoroughgoing exploitation within the Empire itself.

The cynical and ruthless imperialism that developed in the last decades of the Republic was primarily the work of politicians who wished not only to enrich themselves but also to buy the loyalty of armies and the urban plebs by their benefactions at the expense of the conquered.

To one familiar with the field there is little that is new, but the interpretations are for the most part valid, and the myths have persisted and do need to be attacked. Badian blames the Gracchi overmuch for having invented the idea of the exploitation of provinces for the benefit of the city plebs. It is not difficult to find precedents a century earlier. Caesarians among modern scholars will not like Caesar's being called "the greatest brigand of them all." It would not be difficult to find other statements or views that might well be muted or even challenged outright. Nonetheless, the lectures are generally balanced and sound; this is an excellent little volume.

University of North Carolina

HENRY C. BOREN

MARTYRDOM AND PERSECUTION IN THE EARLY CHURCH: A STUDY OF A CONFLICT FROM THE MACCABEES TO DONATUS.
By *W. H. C. Frend*. [Anchor Books.] (Garden City, N. Y.: Doubleday and Company. 1967. Pp. xviii, 577. \$1.95.)

DESCRIBED by the publisher as both "A Doubleday Anchor Original" and "the first publication in the United States" of Dr. Frend's important monograph, the Anchor edition is a reprint, with modifications, of the original edition published with the same title by Blackwell at Oxford in 1965. There is no statement that the author supervised or edited the Anchor edition, but in at least one instance (on page 424, note 41) an additional reference has been added to a footnote. The text of the Anchor edition appears to be unaltered, but many footnotes have been truncated and compressed, and in a number of instances quotations from Greek and Latin writers and from modern scholars have been omitted, leaving only the references to the sources, so that the reader no longer has this evidence conveniently before him. There are numerous typographical errors, especially in Greek and Latin. It is convenient for some purposes to have an inexpensive reprint, but scholars will prefer to use the original edition.

Frend is already known for his valuable study *The Donatist Church* (1952) and for *The Early Church* (1965), which is the best short history for student use now available in English. His comprehensive study of martyrdom and persecution is of special value in a number of respects. Other treatments of the subject have often been written primarily from the viewpoint of the history of the early Church and the religious significance of these developments. Frend's study is based on a broader view of the meaning of martyrdom and persecution not only for the Christian community but as major factors in the contemporary history of the Roman state. To a thorough acquaintance with the Christian sources and the modern studies of them, Frend adds a wide and accurate knowledge of Roman history and an expert knowledge of Roman law, which is particularly valuable in showing what Christian attitudes meant to Roman authorities. There is an extensive and valuable classified bibliography, and the archaeological sources are used fully.

The book's merit and its importance, not only for students of the Roman Empire and of contemporary Jewish history but for students of church history and of church-state relations at later periods, are that it presents a comprehensive and judicious picture of the varied factors—religious, social, and economic—that contributed to the conflict between the Church and the Empire in the first three centuries. One of the greatest values of the book is that it provides a well-balanced account of the rise of Christianity as a social movement. We are reminded that “atheism was the real, damning charge against the Christians,” who, as Gibbon pointed out, were deserting the religion of the society in which they lived.

Indiana University

GLANVILLE DOWNEY

Medieval

FRÜHMITTELALTERLICHE STUDIEN: JAHRBUCH DES INSTITUTS FÜR FRÜHMITTELALTERFORSCHUNG DER UNIVERSITÄT MÜNSTER. Volume I. Edited by *Karl Hauck*. (Berlin: Walter de Gruyter & Co. 1967. Pp. 443, 35 plates. DM 86.)

THE first issue of this handsomely produced new journal contains twelve articles: Karl Hauck, “Von einer spätantiken Randkultur zum karolingischen Europa” (with seven figures); Hans Belting, “Probleme der Kunstgeschichte Italiens im Frühmittelalter” (with twenty-four plates); Victor Elbern, “Theologische Spekulation und die Gestaltungsweise frühmittelalterlicher Kunst” (with four plates); Friedrich Oswald, “Römische Basilika und ottonische Kirche St. Peter auf der Zitadelle in Metz” (with eight figures and two plates); Johannes Karayannopoulos, “Hauptfragen der Byzantinistik der letzten Jahre”; William Foerste, “Der römische Einfluss auf die germanische Fesselungs-Terminologie”; Gunter Müller, “Zum Namen Wolfhetan und seinen Verwandten”; Herbert Jankuhn, “Das Missionsfeld Ansgars”; Karl Schmid, “Über das Verhältnis von Person und Gemeinschaft im früheren Mittelalter”; Otto Oexle, “Die Karolinger und die Stadt des heiligen Arnulf”; Karl Schmid and Joachim Wollasch, “Die Gemeinschaft der Lebenden und Verstorbenen in Zeugnissen des Mittelalters” (with two maps); and Joachim Wollasch, “Ein cluniacensisches Totenbuch aus der Zeit Abt Hugos von Cluny” (with five plates). There are also brief reports on the third colloquium of Château-Gaillard and on the early medieval colloquium held at Münster in April 1966 where six of these articles were presented as lectures.

The volume ranges broadly over medieval and Byzantine history from the fifth to the eleventh century, including art, archaeology, philology, and religion. The articles vary in length from over a hundred to under ten pages; those by Hauck and Oexle are really short monographs. Hauck discusses especially the development of Frankish imperial consciousness and ceremony from the fourth to the eighth century and the work of the Anglo-Saxon missionary “exiles.” Oexle studies in detail the city and church of Metz under the Carolingians. The articles are all serious contributions, and it is hard to pick out others for special mention, but attention may be drawn to those by Schmid and Wollasch because they use a type of source material—monastic confraternity books and

necrologies—that has been neglected by English-speaking historians. These lists of persons for whom monastic prayers were offered are of great value for the study of monasticism, culture, and society (particularly family relationships) from the eighth to the tenth century. In the two final articles Wollasch studies the surviving Cluniac necrologies, especially the so-called necrology of Münchenwiler, which really came from Marcigny and is closely related to the lost necrology of Cluny itself. He shows their importance for establishing who were considered Cluniacs in the eleventh century and how they were active in politics and in the Church.

Harvard University

GILES CONSTABLE

THE CAMBRIDGE MEDIEVAL HISTORY. Volume IV, THE BYZANTINE EMPIRE. Part II, GOVERNMENT, CHURCH AND CIVILISATION. Edited by J. M. Hussey, with the editorial assistance of D. M. Nicol and G. Cowan. (New York: Cambridge University Press. 1967. Pp. xlii, 517. \$14.50.)

THIS part of the revised edition of Volume IV of *The Cambridge Medieval History* is devoted to the institutional and cultural aspects of the Byzantine Empire. As in the case of Part One, it is actually a new work, entirely rewritten by new authorities who are distinguished scholars in their respective field. Thus the late W. Ensslin gives a succinct account of the theoretical basis and actual working of the imperial government; R. Jenkins delves into the social life of the Empire; E. Herman analyzes its ecclesiastical institution; and J. M. Hussey examines monasticism. There are chapters on law, music, theology, literature, science, and art contributed, respectively, by H. J. Scheltema, E. Wellesz, a major figure in the decipherment of Byzantine music, H. J. Hussey with the collaboration of T. A. Hart, F. Dölger, K. Vogel, and A. Grabar. A final chapter by S. Runciman examines the position and role of Byzantium in the medieval world. The bibliography appended covers 194 pages.

There is no question here, of course, of a critical evaluation of each chapter. The authors in their general remarks assess correctly the nature and import of the subject about which they write. To take literature and science as examples: Dölger rightly observes that Byzantine literature, however lacking in freshness and genuine originality, is not "an arid collection of worthless and voluminous outpouring." Byzantine science was indeed Hellenistic science, but Byzantium's role in the history of science was by no means unimportant for, as Vogel remarks, "Byzantium preserved the solid foundation [of science] laid in antiquity until such a time as the Westerners had at their disposal other means of recovering this knowledge." There are, however, matters of detail unlikely to find general acceptance. I think Herman is right in asserting that "it is scarcely correct to define the normal relationship between Church and State in Byzantium by the . . . term" caesaropapism, but others, I am sure, will object. It is hardly possible to agree with Dölger that the epic *Digenis Akritas* is good literature. Ensslin reiterates an old view, long contested by me, that in crowning the emperor the patriarch represented the state and not the church. There are omissions which, in certain instances, mar the pictures drawn by the authors. As an example we may

give Hussey's picture of Byzantine monasticism, its institutions, ideals, practices, and services. The picture is very enlightening, but the author's failure to bring out the abuses of monasticism leaves it incomplete.

The work as a whole is a valuable publication. It makes available, especially for the English reader, considerable information, some of which can be found only with difficulty, if at all, elsewhere.

Rutgers University

PETER CHARANIS

ARCHIMEDES IN THE MIDDLE AGES. Volume I, THE ARABO-LATIN TRADITION. By *Marshall Clagett*. [Publications in Medieval Science, Number 6.] (Madison: University of Wisconsin Press. 1964. Pp. xxix, 720. \$12.00.)

ARCHIMEDES established in antiquity the application to mechanics of deductive methods, an association of mathematics and physics that has been crucial in the historical development of both subjects; yet the place of Archimedes in the Middle Ages has until recently remained relatively obscure. It is true that the author-editor of this welcome and impressive volume has been publishing articles, especially since 1952, in *Isis* and *Osiris*, disclosing that Archimedes was not unknown in Western Europe after the twelfth century; but now scholars will have at hand the full weight of the evidence, all of it conveniently arranged with English translation facing the Latin text. In addition, one has introductions and commentaries that are especially commendable, for they are not devoted to minor points in translation or to variant readings in the manuscripts—matters adequately handled in footnotes and other scholarly apparatus—but serve rather to set the material in broad perspective or to provide helpful explanations of the meanings of passages.

A reader accustomed to thinking of the Muslim world as the preserver of ancient science may be rudely surprised to learn of the scarcity of Archimedean texts available to the Arabs. Distinguished by their absence are the works *On Spiral Lines*, the *Quadrature of the Parabola*, the *Conoids and Spheroids*, the *Sandreckoner*, and *On the Method*. Most of this very substantial volume, comprising the Arabo-Latin tradition, is devoted to the text and translation of but two Archimedean works: *De mensura circuli* and *De sphaera et cylindro*. The first of these is represented here by more than a half-dozen versions, some never before printed; of the *Sphere and Cylinder* only several large fragments are available from the Arabic stream, and Clagett has handled these in the same thorough manner as the *Measure of the Circle*. The phrase "Archimedean treatise" has been broadly interpreted to include Arabo-Latin works clearly inspired by the great Syracusan, even though not necessarily in his own words. Moreover, the volume has been enriched by a half-dozen appendixes containing medieval texts and translations related to topics in the two Archimedean treatises in question, such topics as circle quadratures of the thirteenth century, the quadrature of lunes, Heron's formula for the area of a triangle, and angle trisections of Jordanus and Campanus.

Physicists may be disappointed with this first volume of Clagett's monumental study, for "the influence of the Arabic tradition of Archimedes' physical works

on Latin science" was "much more remote and indirect" than that of the mathematical treatises. For medieval translations into Latin of the *Equilibrium of Planes* and *On Floating Bodies*, as well as further texts in mathematics, one eagerly awaits Volume II, which is to contain the corpus of translations from the Greek text, notably by William of Moerbeke in 1269. The second volume promises to be more attractive for some readers, but this in no way detracts from the significance of the first, the concluding sentence of which reminds us that "The continuing citation and copying of these treatises translated from the Arabic takes us down into the Renaissance, when they were finally displaced by a renewed interest in the Greek text."

Brooklyn College

CARL B. BOYER

ARABISCHE QUELLEN ZUR CHRISTIANISIERUNG RUSSLANDS. By Peter Kawerau. [Osteuropastudien der Hochschulen des Landes Hessen. Second Series, Marburger Abhandlungen zur Geschichte und Kultur Osteuropas, Number 7.] (Wiesbaden: Otto Harrassowitz. 1967. Pp. x, 73. DM 14.)

HERR Kawerau's monograph sheds a brighter and more concentrated, if not a new, light on one of the most important events in the cultural and religious history of Russia: its conversion to Christianity. The Arabic sources cited, together with some materials drawn from non-Arabic (Syriac, Armenian, and Persian) sources, give little more than tangential data on the event itself, but they do prove, however, that Arabic and Arabic-influenced historians and geographers had an interest in the Slavo-Norse Russians of the tenth century. This interest, fed by trading contacts in the Caspian area, was known to the scholars who launched modern Russian research into their own medieval beginnings, and such scholars as V. V. Bartol'd, Baron Rosen, and A. A. Vasiliev came naturally to combine Byzantine, Russian medieval, and Arabic studies. Many Russian editions and translations, however, remain in Russian, and Kawerau's work is intended to make a thin but important slice of Arabic commentary available to non-Russian readers.

Most of the larger excerpts in Kawerau's monograph deal with the events of 987-988, when, in an elaborate series of negotiations, Vladimir of Kiev traded six thousand mercenaries to Emperor Basil II in return for a purple-born princess, and had to throw in the souls of his people to secure the bargain. Some Arab writers had a vague idea that Christianity had penetrated Russia earlier; some did not. The accuracy of the information the Arabs had on the conversion is not especially marked. The event is simplified and dramatized: Vladimir himself, in the narrations of Ibn al-Athir and Al-Makīn, led the Russian mercenaries, and the mass baptism of the Russians precedes the dispatch of troops to help Basil put down the great rebellion of Bardas Phocas. Nevertheless, the Arabs give us at least as much information as the Greeks, and even the misinformation they pass on can be significant and helpful. Kawerau's work is certainly not wasted: Byzantinists and medievalists now have a small, perhaps too small, solid extension of translated materials useful in attacking both the problems bearing on the beginnings of Russian Christianity and on the nature of Arab curiosity and

knowledge about this peripheral part of their world. Kawerau's monograph can be mined in the same fashion as Vasiliev's *Byzance et les Arabes*, Minorsky's *Hudud al-Alam* and, in a slightly different context, Irène Sorlin's study of the Byzantine-Russian treaties of the tenth century.

The book is heavily and well annotated and has a bibliography complete in all important respects. Arabic, but not non-Arabic, texts are included for those who have the equipment for textual analysis.

University of Rochester

D. A. MILLER

BULLETIN PHILOLOGIQUE ET HISTORIQUE (JUSQU'À 1610) DU
COMITÉ DES TRAVAUX HISTORIQUES ET SCIENTIFIQUES.
ANNÉE 1963: ACTES DU 88^e CONGRÈS NATIONAL DES SOCIÉTÉS
SAVANTES TENU À CLERMONT-FERRAND. In two volumes. [Min-
istère de l'Éducation nationale.] (Paris: Bibliothèque Nationale. 1966. Pp.
lvii, 483; 486-1026.)

It has been customary of recent years for the *Comité des travaux historiques et scientifiques* to designate a central problem for discussion at the history section of the annual *Congrès national des sociétés savantes*. Forests were the central topic of the 1963 meeting at Clermont-Ferrand. Some twenty papers, nearly all of the first volume, dealt with the forests of medieval and early modern France. Most of the papers concentrate on a single forest, locality, or region: Villepreux, Chizé, Grésigne, Héric, Languedoc, Picardy, and others. On the valid assumption that forests were vastly important in an age that relied on wood for both construction and daily needs, the authors of these papers have re-examined methods of exploitation, fluctuating geographical coverage, administrative procedures, and the relation of forests to legal and economic developments.

The papers, naturally, are uneven. Some merely add details to a picture already well known. Others, notably those by Heinrich Rubner, Henri Gilles, Monique Hébert, and Georges Plaisance, are of fundamental importance. While not entirely superseding the standard study of French forests by Alfred Maury (1867), the present volume will unquestionably be the point of departure for all subsequent work on this aspect of French economic, agricultural, and demographic history.

The remaining thirty papers deal with a variety of topics, but the emphasis is clearly economic and social history. Among the significant contributions worth noting are Jean-Jacques Hémardinquer, "L'introduction du maïs et la culture des sorghos dans l'ancienne France"; Madeleine Saint-Eloy, "Des échevins de Nevers, 1309-1610: Condition sociale, fortune et profession"; André Leguai, "L'économie bourgonnaise pendant la guerre de Cent Ans"; and B. A. Pocquet du Haut-Jussé, "De la vassalité à la noblesse dans le duché de Bretagne." In addition to three papers on aspects of paleography or diplomatics we find an important study of the administrative movement of *baillis* and *sénéchaux* in the early fourteenth century, a detailed narrative of the role of the provincial estates of Basse Auvergne between 1589 and 1594, and an analysis of Sully's substantial achievement in developing French artillery.

All of the papers are not equally significant, but surprisingly few are insubstantial. There is enough here to provide nourishment for serious students of medieval and early modern French history. There is also enough, it may be added, to demonstrate the virility of current French historical scholarship.

University of Utah

DAVIS BITTON

XII^e CONGRÈS INTERNATIONAL DES SCIENCES HISTORIQUES.
ÉTUDES PRÉSENTÉES À LA COMMISSION INTERNATIONALE
POUR L'HISTOIRE DES ASSEMBLÉES D'ÉTATS. Volume XXXI,
WIEN, 1965. (Louvain: Éditions Nauwelaerts. 1966. Pp. xii, 198, 41. 480
fr.B.)

THIS volume contains the "reports" presented to the International Commission for the History of Representative and Parliamentary Institutions when it met during the Twelfth International Congress of Historical Sciences held in Vienna in 1965. The studies cover a geographical area embracing England, France, Hungary, the Byzantine Empire, Austria, and Bohemia; their authors represent the United States, Great Britain, France, Belgium, Hungary, Cyprus, Austria, the Soviet Union, and Czechoslovakia.

These papers vary considerably in significance. Many are merely brief reports to the commission and offer nothing essentially new. A few, however, make a contribution in their own right to the study of parliamentary institutions. Thomas N. Bisson's "Negotiations for Taxes under Alfonse of Poitiers" indicates clearly that the intensive study of a restricted area during a limited time period may well contribute much to our understanding of the development of representative institutions on a larger national scale. The paper by C. P. Kyrris on "Representative Assemblies and Taxation in the Byzantine Empire between 1204 and 1341" points up the curious similarity and remarkable dissimilarity between Byzantine "representative" institutions and those of Western Europe. The paper by Raymonde Foreville on "Représentation et Taxation du Clergé au IV^e Concile du Latran (1215)" suggests that a continued study of the development of clerical assemblies still has much to contribute to an understanding of secular assemblies. And Peter Spufford's "Assemblies of Estates, Taxation and Control of Coinage in Medieval Europe" is an interesting attempt at comparative history. In this case Spufford traces the relationship between the growth of representative institutions and the changing attitude toward coinage (from royal control to community control) as these developed in Aragon, Normandy, Toulouse, Burgundy, England, Brabant, and France.

Collectively the papers brought together in this volume emphasize to the interested student of representative institutions those areas where progress seems to have come to a virtual standstill, at least until essentially new documentary material is discovered, and those areas where much still remains to be done with the materials already available to the world of scholarship. From this standpoint, the present volume is useful.

Rice University

K. F. DREW

DE OORKONDEN DER GRAVEN VAN VLAANDEREN (1191–AANVANG 1206). Volume I, DIPLOMATISCHE INLEIDING. By *W. Prevenier*. [Koninklijke Academie van België, Koninklijke Commissie voor Geschiedenis. Verzameling van de akten der Belgische vorsten, Number 5.] (Brussels: the Academie. 1966. Pp. xxiii, 630. 840 fr. B.)

THIS is Volume I of a series of three volumes. The second and principal volume containing 298 charters, or diplomas, of the counts of Flanders from 1198 to 1202 has already appeared, and the third volume, which will include additional documentation and the indexes, is yet to come. The second volume was very favorably reviewed here by Professor Gray C. Boyce (*AHR*, LXXI [July 1966], 1311). This first volume, although it is more than six hundred pages long, is devoted entirely to a diplomatic introduction that is thorough, and perhaps more elaborate than necessary. It deals exhaustively with the practices of the Flemish chancery and the style, script, dating, composition, and arrangement of the diplomas, or charters, issued during the reigns of Baldwin VIII and IX. The latter, let us recall, became Latin emperor of Byzantium in 1204, so that his chancery was divided between Flanders and Constantinople, and in both places charters for the counties of Flanders and Hainaut were issued. Some of these documents may interest Byzantinists.

Since diplomatics is an auxiliary science that is rather neglected in this country, this introduction would have been very useful in the hands of aspiring medievalists if it had been written in a language that most scholars could read. It is, therefore, a great pity that the author, who knows French perfectly well, chose to write in Flemish-Dutch, a language familiar to few persons outside of Belgium and the Netherlands. Nationalism thus turns out to be detrimental to culture. If the author believes that the Flemish people eagerly read treatises on diplomatics, he is very much mistaken. Since his publication interests only specialists scattered all over the world, it should have been written in an international language. It is waste of public funds—the publications of the *Commission royale d'Histoire* are paid for by the Belgian government—to issue expensive volumes, beautifully printed and well bound in cloth, that fail to fulfill their purpose and to promote the knowledge of Belgian history abroad as well as at home. This is *esprit de clocher* at its worst.

Brooklyn College

RAYMOND DE ROOVER

A SOCIAL AND RELIGIOUS HISTORY OF THE JEWS. LATE MIDDLE AGES AND ERA OF EUROPEAN EXPANSION, 1200–1650. Volume IX, UNDER CHURCH AND EMPIRE; Volume X, ON THE EMPIRE'S PERIPHERY; Volume XI, CITIZEN OR ALIEN CONJURER; Volume XII, ECONOMIC CATALYST. By *Salo Wittmayer Baron*. (2d rev. ed.; New York: Columbia University Press; Philadelphia: Jewish Publication Society of America. 1965; 1965; 1967; 1967. Pp. x, 350; 432; 422; 359. \$8.50; \$8.50; \$10.00; \$10.00.)

PUBLICATION of these volumes brings Baron's monumental history of the Jews to the threshold of the modern age. The four volumes, constituting a unit

within the larger history and titled *Late Middle Ages and Era of European Expansion*, recount the saga of the Jews from the beginning of the thirteenth to the middle of the seventeenth century.

Those familiar with the previous volumes of Baron's history know that his is an undertaking and achievement *sui generis*. No other contemporary Jewish scholar has attempted so ambitious a project, comparable only to the works of Graetz and Dubnow in their respective generations and cultures, and, amazingly, no cooperative effort at writing a history of the Jews has, to date, advanced as far as Baron's. The solitariness of Baron's work evidences the incomparable complexity of Jewish history and hence of Jewish historical research. Jewish history possesses characteristics unlike that of any other group. Almost from its beginning, it has been confined to no single land mass, polity, or culture. In nearly all epochs it has developed simultaneously in different locales, each Jewish enclave participating appreciably if not fully in the life of its host society, and each usually capable of exhibiting a distinctiveness from its siblings not only in the social, economic, and political spheres but also in ideological and cultural development. Narration of the history of the Jewish groups has therefore always entailed a hazardous enterprise. The historian has to limn the history of the separate Jewish communities, and, at the same time, to fathom the unmistakable elements of Jewish unity transcending all division and even divisiveness. These elements are a common tradition or ideology, a sense of peoplehood, and, resulting from these, a propensity for similar experiences.

At the same time, the solitariness of Baron's achievement eloquently testifies to his comprehension of the scope of Jewish history and his ability to communicate it significantly to the modern reader.

As do the previous tomes of the history, these volumes display Baron's meticulous concern for details and an exhaustive bibliographical apparatus covering the entire field of Jewish studies and the most significant of the related works from other fields. They also follow the topical approach, with due regard for chronology and treatment of personality. Each of the four volumes covers the same period. Each contains thorough analyses and discussions of diverse topics. Thus Volume IX, bearing the title *Under Church and Empire*, includes discussions on the Jews as *servi camerae*, their disputations with Christians, the compulsory sermons and thought control to which they were subjected, and, in general, the "uneasy coexistence" of the medieval Jew and his neighbor. Volume X focuses on such topics in the various countries *On the Empire's Periphery*, including "Germany's neighbors," that is, the Low Countries, Switzerland, Hungary, and Poland, and the more distant lands of England, the Italian area, and the Iberian Peninsula.

Volume XI moves even closer to the internal life of the Jewish community with its treatment of the authority structure within the Jewish community and the effect of impositions from outside upon the community, including the ghetto, the Jewish badge, the oath *more judaico*, and libels like ritual murder, well poisoning, and host desecration leveled against them. It discusses as well the expulsions and wanderings of the medieval Jew. Volume XII concentrates on the economic activities of the Jews, including landowning, crafts and guilds, medicine, diplomacy, commerce, and moneylending, and, in general, the economic role of

the Jews in strengthening the medieval polities to which they belonged and in helping to lay the foundation for the new world that emerged from them.

The mosaic of these topics, skillfully woven, yields the most comprehensive work yet written on the Jewish Middle Ages, and an indispensable reference for all serious scholarship in the field, not only Jewish but general as well. General scholarship can profit by a greater understanding of the seminal role, active as well as passive, played by the Jews in nearly every sphere of medieval development.

Though scholars may quibble with scattered details or conclusions in Baron's work or express a preference for different criteria of selectivity and periodization, few will fail to use Baron's new volumes without profit, and even fewer will seek to equal his achievement.

Hebrew Union College

MARTIN A. COHEN

RECUEIL DES ACTES DE PHILIPPE AUGUSTE, ROI DE FRANCE.

Volume III, ANNÉES DU RÈGNE XXVIII À XXXVI (1^{er} NOVEMBRE 1205–31 OCTOBRE 1215). By J. Monicat and J. Boussard. Published under the direction of Charles Samaran. [Chartes et diplômes relatifs à l'histoire de France.] (Paris: Imprimerie Nationale. 1966. Pp. 555.)

THE first volume of the charters of Philip Augustus was edited in 1916 by Delaborde; the second by Delaborde, Petit-Dutaillis, and Monicat in 1943. Publication of the third by Monicat and Boussard sets a pace of quarter-century intervals and invites discouraging conjecture about when the fourth and perhaps final volume will appear. It also speaks of the passing of generations of scholars, and we have yet to see the first volume of charters from the reigns of Saint Louis and Philip III.

A collection of this kind lends itself to statistical observations: Of the 442 acts edited here, 102 are extant as original charters, and 70 are not cited in Delisle's *Catalogue*. Also, 167 of these charters were heretofore unpublished; of this number 38 have never been cited, and presumably never used, in published works, and 12 of these are extant as original charters, possessing what might therefore be called rather certain diplomatic authenticity. This last number seems to be the ultimate statistical *reductio*, but it has little significance otherwise. Perhaps the most impressive figure is the 167 unpublished acts, but the real importance lies as always in the collection itself, in the gathering of all these charters, published and unpublished, into one volume.

The acts range in content across most of the issues that held the attention of monarchs in the early thirteenth century. There are nine charters for French communes, and several deal with urban unrest. Negotiations between Church and state are prominent, and other topics treated include serfdom, money-changers, Jews, Templars, military service, Normandy, Champagne, and the omnipresent land grants and gifts of revenue.

While an excellent example of the scholarship of the French school of diplomatists, this volume has one grave defect. Like the preceding two, it has no index, which, at the outset of the project, was postponed until completion of the series. Meanwhile, fifty years have passed, and there is no means yet of easy access

to the wealth of material in the three volumes with their fourteen hundred charters.

Ohio State University

FRANKLIN J. PEGUES

FLORENCE IN TRANSITION. Volume I, THE DECLINE OF THE COMMUNE. By *Marvin B. Becker*. (Baltimore: Johns Hopkins Press. 1967. Pp. 263. \$7.50.)

IN this analysis of Florence's political experience during the first half of the fourteenth century the author attempts to define the social composition and policies of the communal government, the crises and responses to crises, the significant political trends, and also the relationship between politics and culture in these decades. Becker argues that the communal fisc is the key for understanding political change in *trecento* Florence. Underlying the transformation of the communal structure was a fiscal crisis that reached a climax in the 1340's, and that, ultimately, led to intensified exploitation of rural and urban resources, the suppression of privileges and immunities, and the imposition of more stringent controls upon society. There is much validity in this analysis, although by placing so much emphasis upon the fisc the author tends to neglect the influence of other forces in this process. I have some reservations about the reliability and completeness of his fiscal data. The commune's judicial and fiscal records, while not always complete, have survived since 1343, and evidence concerning the fisc and justice before this date is very fragmentary. Because the chapters on the crisis years of the 1340's are more solidly documented than those devoted to the earlier decades, they constitute the most useful section of the book.

In Chapter 1 the author develops a conceptual framework for his analysis that unfortunately weakens the book and detracts from its achievement. His categories and terminology are misleadingly borrowed from ancient Greece. Prior to 1340, he argues, the Florentine "polis" was a loose and relaxed regime—a "gentle paideia"—which relied upon suasion rather than force. After 1340 this tolerant, laissez-faire government was replaced by a "stern paideia" characterized by impersonal rule and a puritanical ethos. Becker's attempt to force the complex historical experience of Florence into this rigid Procrustean format seems highly dubious to me. It minimizes the continuities in Florentine politics and contradicts some of the author's own findings pertaining to crisis in Chapter 11. Throughout the fourteenth and fifteenth centuries the commune tended to be more tolerant in the fiscal and judicial spheres during periods of prosperity and internal peace, and more rigid in times of crisis. I am not persuaded by Becker's argument that the optimistic belief in man's capacity for virtue was transmitted to the political arena and became the ideological foundation for the mild and hortatory regime of the "gentle paideia" which, so he asserts, was deeply committed to individual freedom. This thesis ignores the brutal realities—the bitter partisan conflicts, the political executions and exiles—of the Age of Dante, of which the poet himself was witness and victim.

University of California, Berkeley

GENE A. BRUCKER

RECUEIL DE DOCUMENTS RELATIFS À L'HISTOIRE DE L'INDUSTRIE
 DRAPÈRE EN FLANDRE. Part 2, LE SUDOUEST DE LA FLANDRE
 DEPUIS L'ÉPOQUE BOURGUIGNONNE. Volume III (LOCRE-WOR-
 HOUDT—SUPPLÉMENT). By *Henri-E. de Sagher*. Published under the
 care of *Johan-H. de Sagher et al.* (Brussels: Académie royale de Belgique,
 Commission royale d'Histoire. 1966. Pp. xii, 618. 820 fr. B.)

THESE volumes of documents concerning the Flemish cloth industry are a continuation of the collection published by Henri Pirenne and Georges Espinas, which stops in 1384 when the Burgundian dynasty came to power. Unlike that collection, which covered the whole of Flanders and Artois, this new series deals with only the southwestern part of the former county of Flanders, the region surrounding Ypres, which includes towns on both sides of the present Franco-Belgian frontier. The publication is limited to the smaller manufacturing towns and villages and consequently excludes Ypres and Lille, but not Tourcoing. In other words, only the "rural" cloth industry since 1384 is dealt with. During this period the principal cloth-producing centers were: Armentières, Bergues-Saint-Winoc, Comines, Diksmuide, Hondschoote, Meenen, Messines, Neuve-Église, Poperinge, and Wervicq. At the instigation of Pirenne, the documents were collected by Henri-E. de Sagher who died prematurely in 1940 at the age of fifty, leaving the difficult task unfinished. The work was completed by his son with the assistance of Professor Hans Van Werveke and Carlos Wyffels, archivist of Ghent, who added more documents and brought the introductory notices up to date. The one on Wervicq, for example, was considerably revised.

The arrangement is alphabetical according to place names; for each town there is a brief notice on the importance of its woollen industry, followed by the documents, usually published in full. A fourth volume will contain the statistical tables and index. Most of the documents are public regulations—since the rural cloth industry was not organized in guilds—and papers regarding lawsuits, especially with Ypres, which claimed a monopoly and sought to prevent the manufacture of cloth in neighboring rural areas. The rural industry of West Flanders prospered in the fifteenth century, then declined, and recovered somewhat in the sixteenth century, but only in a few centers. After 1580 the decline was precipitous, although the industry was slow to die and a few looms were still active in the eighteenth century. These volumes will be invaluable to anyone working on the Flemish cloth industry.

Brooklyn, New York

FLORENCE EDLER DE ROOVER

ROYAL AND HISTORICAL LETTERS DURING THE REIGN OF
 HENRY THE FOURTH, KING OF ENGLAND AND FRANCE AND
 LORD OF IRELAND. Volume II, A.D. 1405–1413. With amendments,
 entered in calligraphy, supplied by the Public Record Office. Edited by *F. C.
 Hingeston*. (London: H. M. Stationery Office; distrib. by British Information
 Service, New York. 1965. Pp. lxxix, 403. \$28.00 postpaid.)

THIS volume, published in 1965 by the Public Record Office, was originally prepared for publication in 1865 in the Rolls Series in which Volume I appeared

in 1860. After the volume was ready, the text was discovered to contain many inaccuracies. Rather than reset the whole or publish a long list of errata, it was decided to withdraw the work and destroy all but eight copies. For the present edition, amendments were made in the original text in longhand, and the work was photographically reproduced. In addition to the letters, the volume contains the original editor's preface to this volume and the glossary and index for both volumes, printed for the first time.

In his preface the editor discussed most of the letters under the country or subject that they concern. For some he provided considerable supplementary material. A chronological summary serves as a table of contents. Letters in French, Spanish, and Portuguese were printed with translations; letters in Latin and English only in the original. For the discussion of manuscript sources, comments on texts, and the like, one must refer to the preface to Volume I.

The letters, like those in the first volume, chiefly concern external affairs, negotiations with the Scots and unrest in the north, negotiations with France and Flanders, and relations with Prussia and the Hanseatic towns. Four letters, all written in 1405, deal with the Welsh rebellion. An interesting group, to whose dating the editor devoted considerable attention, relate to the marriage of Thomas, earl of Arundel, to the illegitimate daughter of the King of Portugal. Other letters concern Ireland, Spain, dangers caused by pirates in the English Channel, the appointment of justices, and ecclesiastical affairs, particularly Henry's interest in ending the papal schism after the death of Innocent VII.

The majority of the letters were addressed to the King or the council; approximately one-fourth were written by the King. One, in French, has a closing sentence in English, initialed H. R., which the editor says is in the King's hand. The six letters written by Prince John, Henry's third son, indicate John's involvement in negotiations with Scotland and the troubles in the north. The Prince of Wales is represented by a single letter in which he asked his father for aid against the Welsh. Thomas, duke of Clarence, who replaced the Prince of Wales in negotiations with the French, wrote in behalf of his father in regard to the French alliance.

With this volume, the Public Record Office has completed a bit of unfinished business and made available in print some sources for the history of England in the early fifteenth century.

Princeton, New Jersey

ELISABETH G. KIMBALL

A HISTORY OF THE HUSSITE REVOLUTION. By *Howard Kaminsky*. (Berkeley and Los Angeles: University of California Press. 1967. Pp. xv, 580. \$15.00.)

HISTORIANS unfamiliar with the Czech language owe a large debt of gratitude to Howard Kaminsky for his scholarly and objective volume on Hussitism. In his view this movement was both a reformation and a revolution, the most significant revolution of the late Middle Ages. Although this view is not new, his approach is. Inspired by inquiries of the scholarly world and influenced to a large degree by Marxist historiography, he took a fresh look at all the relevant sources and came to the conclusion that Hussitism was a fundamental challenge to the old Euro-

pean order—political, economic, social, and cultural, as well as religious. Reformation and revolution are for him a single process. For this reason he concentrates on the period between 1414, when the conjunction of the two forces took place, and 1424, when both ceased their interaction and creativity. The movement, he points out, reached its height in the Taborite brotherhood.

Kaminsky does not provide us with a balanced narrative of events such as Frederick Heymann's *John Žižka and the Hussite Revolution*. Instead, he supplements this narrative by an analytical reconstruction of the revolutionary process with an emphasis on ideas, programs, and doctrines, making the sources tell their own story in as much detail as possible. Consequently he makes use of an elaborate footnote apparatus for lengthy digressive discussions of his sources and variations in the interpretations of historians, hoping that this now unpopular device will prove useful both to specialists and those unfamiliar with the Czech language.

After a thorough discussion of the backgrounds of Hussitism in the fourteenth century, including the contributions of Wyclif and Huss and the emergence of the Hussite left, Kaminsky analyzes in detail the Utraquist revolt of 1414-1415, the establishment of the Hussite League and its conflict with the left, the emergence of the Taborites and the change from reformation to revolution in 1419, a definition of the Taborite society, the defeat of Emperor Sigismund, and the disastrous internal wars. Especially helpful and novel are the insights he provides on the role played by Master Jakoubek of Stříbo, first as a radical, later as an anti-Taborite, but always as a statesman of a national reformation.

This book's usefulness is enhanced by the inclusion of an excellent bibliography, a carefully prepared index, photographs and maps, and three appendixes: the anonymous anti-Utraquist treatise, *Estote sine offensione*; a discussion of the Taborite synod in Klatovy and related problems; and treatises on Adventism, chiliasm, and warfare.

Ohio State University

HAROLD J. GRIMM

LE TERRIER DE JEAN JOSSARD, COSEIGNEUR DE CHÂTILLON-D'AZERGUES, 1430-1463. Published by *René Fédou*. [Ministère de l'Éducation Nationale, Comité des Travaux Historiques et Scientifiques, Section de Philologie et d'Histoire, jusqu'à 1610. Collection de documents inédits sur l'histoire de France, Series in-8°, Volume V.] (Paris: Bibliothèque Nationale. 1956. Pp. 160.)

In late medieval France the lord of the countryside often required that his tenants recognize before legal witnesses what goods, possessions, and tenures they held of him, and that they promise by oath, for themselves and their heirs, to pay the established dues and obligations to him and his heirs. The recognitions and oaths were recorded by notaries who were official representatives of a legal tribunal, and when put together in the form of a register were called a *terrier*. Because it bound both lord and tenant to the contract and could be used in court, it differed sharply from the *censiers* of the thirteenth century, which were simply collections of rents and obligations for the administrative convenience of seigneurs and their bailiffs. The *terrier* marked the end of an evolutionary development of manorial

registers that began with the Carolingian *polyptiques* and symbolized for the peasant of 1789 the *ancien régime*.

The *terrier* of Jean Jossard is significant because it is one of the few lay registers surviving from the late medieval period, its time span covers the period of agricultural ruin and recovery at the end of the Hundred Years' War, and it comes from the countryside northwest of Lyons, an area that has not yet received the close attention of economic historians. Having inherited part of the seigneurie of Châtillon-d'Azegues on a western tributary of the Saône, Jossard decided in 1430 to re-establish his rents and dues in the wake of dislocation caused by war and the roving companies. Of eighty-three tenants responding from eight parishes, all were free. Most were simple farmers, many were artisans, and some few were notaries. Like other lords, Jossard, finding many tenures abandoned, frequently reduced rents in order to retain manpower.

Professor Fédou is clear about the value of the *terrier* for rural history: it provides a minute description of the countryside and of the structure and obligations of manorial tenures, and it gives precise testimony on the state of the agricultural economy. He has published the full text of the first eleven folios and omitted the recurring formulas of the remaining sixty-seven while using abbreviations for economy of space. An excellent and lengthy introduction, a detailed index, two maps, and three plates make this a valuable book.

Ohio State University

FRANKLIN J. PEGUES

Modern Europe

LE XVI^e SIÈCLE EUROPÉEN: ASPECTS ÉCONOMIQUES. By *Frédéric Mauro*. ["Nouvelle Clio": L'histoire et ses problèmes, Number 32.] (Paris: Presses Universitaires de France. 1966. Pp. 387. 22 fr.)

ANOTHER volume in the "Nouvelle Clio" series, this is the sequel to Jacques Heers's book on the economic history of the fourteenth to the fifteenth century (*AHR*, LXIX [Jan. 1964], 503). It is similarly divided into three parts: sources, what we know, what remains to be done. Most outstanding and useful is the very complete and well-classified bibliography of more than seventeen hundred items on the economic history of the sixteenth century. A less commendable, or perhaps more controversial, feature is the intrusion of economic growth theory and pseudoscientific terminology made fashionable by W. W. Rostow and others. The use of this jargon contributes nothing to clarity, and the author would have done better to adhere closer to the language of Voltaire. The uncritical application of theory to historical conditions is even more debatable. Thus, the author tries by means of formulas to figure out the velocity of monetary circulation in the sixteenth century. I do not doubt the obvious fact that money circulates, but how fast is another matter. Since economists have not been able to determine today's velocity, how is the historian going to supply a figure for the sixteenth century? To answer unanswerable questions is not the task of history, but belongs to the realm of idle speculation. This intrusion of theory is revealed in both the arrangement of the book and the titles of chapters

in Part II: "Supply," "Demand," and "The Interplay of Supply and Demand." The author is widely read and well informed about the state of research; he has used his extensive bibliography to the best advantage. The chapter on methodological problems is well worth reading, although it exhibits a great deal of concern with the quantitative approach and accepts such questionable interpretations as Earl J. Hamilton's theory of progress due to profit inflation and wage lags. On the other hand, the author reviews fairly the current debate about the use of credit instruments in which I am involved. Was the bill of exchange primarily a transfer or a credit instrument? It could, in fact, be used for either purpose or even for both purposes combined; it is difficult to formulate any rule as the circumstances varied from case to case.

On the whole, despite its controversial character, or perhaps because of it, this is a thought-provoking book which will be useful to instructors and students alike.

Brooklyn College

RAYMOND DE ROOVER

LE RELAZIONI DIPLOMATICHE FRA L'AUSTRIA E IL GRANDUCATO DI TOSCANA. Third Series, 1848-1860. Volume I (28 MARZO-29 DICEMBRE 1849). Edited by *Angelo Filipuzzi*. [Documenti per la Storia delle Relazioni Diplomatiche fra le Grandi Potenze Europee e gli Stati Italiani 1814-1860. Part 2, Documenti Esteri. Fonti per la Storia d'Italia.] (Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1966. Pp. xv, 463. L. 4,000.)

LE RELAZIONI DIPLOMATICHE FRA LA GRAN BRETAGNA E IL REGNO DI SARDEGNA. Third Series, 1848-1860. Volume III (2 GENNAIO 1850-24 GENNAIO 1852). Edited by *Federico Curato*. [Documenti per la Storia delle Relazioni Diplomatiche fra le Grandi Potenze Europee e gli Stati Italiani 1814-1860. Part 2, Documenti Esteri. Fonti per la Storia d'Italia.] (Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1966. Pp. xv, 420. L. 4,000.)

THESE important volumes significantly illuminate Austrian and British policy in Italy and domestic questions in the grand duchy of Tuscany and the kingdom of Sardinia after the failure of the revolutions of 1848-1849. To a lesser degree they afford glimpses of French policy. Both volumes are edited by highly competent scholars, and few flaws can be found except for several typographical errors in the collection of British documents.

The volume on Austro-Tuscan relations deals with the successful establishment of Austrian military power in Tuscany and with Austrian efforts to stamp out liberalism and constitutional government. Relations may have improved slightly by the end of 1849, but there was deep distrust between Florence and Vienna. The Austrians regarded Leopold II as weak and incompetent, the press as subversive, the ministers as generally weak except for Interior Minister Leonida Landucci, and any possible meeting of the chambers as a revolutionary threat. The Austrian military presence was unpopular with many Tuscans, and there were long negotiations before a military convention was concluded in 1850. The Grand Duchess was apparently more favorable to the Austrians

than her husband, and one of her ambitions was to have the heir brought up at Vienna. Tuscany lived under the constant threat that Austria would proclaim martial law. Tuscany maintained close relations with the kingdom of Sardinia. Relations between Tuscany and Britain were cool. Austria worked to prevent any serious friction from developing between Rome and Florence. In addition to the regular diplomatic correspondence between the Austrian legation at Florence and Vienna, the pertinent correspondence between Prince Felix Schwarzenberg, Austrian president of the council and Minister of Foreign Affairs, and the Austrian military leaders in Italy is reproduced.

The Curato volume is based on material in the Public Record Office, but the editor has also consulted the Royal Archives at Windsor and the Palmerston Papers. Relations between Britain and the kingdom of Sardinia in the period covered by this volume were warm and intimate. Ralph Abercromby, the British minister at Turin, was a clearheaded diplomat who saw the key to Italy's future in a strong, prosperous, and well-governed Piedmont. The British viewed the kingdom of Sardinia as the only effective barrier to complete Austrian domination of Italy. Abercromby believed that Austria was attempting to create an excuse for attacking the small country. He and Lord Palmerston, the British Secretary of State for Foreign Affairs, gave moral and diplomatic support to the government at Turin, but sought to calm its excessive fears. Britain fostered satisfactory relations between Piedmont and neighboring states, especially Austria and the papacy. Britain encouraged the initial planning for railway connections between Piedmont, Switzerland, and Prussia. Prime Minister and Foreign Minister Massimo d'Azeglio had great confidence in the British and much admiration for Lord Palmerston. Only on the question of seizure of territory from Monaco did Palmerston refuse to support Piedmont. There was a strong tendency in the kingdom of Sardinia to regard France as an accomplice of Austria in the attempt to crush liberal institutions in Italy, and French influence at this time was waning at Turin.

Colgate University

WILLIAM C. ASKEW

STORIA DELLE RELAZIONI COMMERCIALI TRA L'ITALIA E LA FRANCIA DAL 1860 AL 1875. By *Luigi Izzo*. [Istituto Universitario Navale di Napoli, Facoltà di Economia Marittima, Istituto di Storia del Commercio. Collana di storia del commercio, Fonti e ricerche, Number 1.] (Naples: Edizioni Scientifiche Italiane. 1965. Pp. xxvii, 452. L. 5,000.)

THIS large and handsome volume has the honor of initiating the new series in the history of trade under the auspices of the faculty of naval economics at Naples. If the outlines of the story are familiar, particularly the recriminations to which united Italy's commercial policy gave rise, the record has never before been set down so fully and so dispassionately though with certain clearly announced convictions. Basing his account firmly on French and Italian governmental records, both archival and printed, Izzo first surveys the economic situation in France and in the several Italian states on the eve of unification; he then follows their fluctuating trade relations during the next fifteen years.

His general theme is simply stated: in the 1850's the kingdom of Sardinia,

hoping to gain political and military support in a future conflict with Austria, granted large trade concessions to the French. In its turn, united Italy, to show its gratitude and good faith, extended those concessions to include the whole of the new state. This meant that France received quite unwarranted advantages that amounted to an economic hegemony over most of Italy—an Italy, with the exception of Sardinia, unarmed technically and financially. At a slightly later date northern Italy gained tariff protection against the outside world and at the same time established its economic domination over the rest of the kingdom.

This is not at all to say that the book is a long polemic against the villainous French and the unscrupulous men of Turin, Genoa, and Milan. One almost wishes that it were when one arid page of statistics follows another or when one long list of exports or imports repeats almost exactly a similar list for an earlier period. A different organization of his material would have been more illuminating and less taxing on the reader.

Heavy as the going at times is, this is a highly useful work of reference for only a few of our largest libraries possess the original volumes from which the data have come. And Mr. Izzo has assembled them in easily usable form, providing an excellent bibliography but a somewhat inadequate index (proper names only).

University of Vermont

PAUL D. EVANS

STORIA DIPLOMATICA DELLA QUESTIONE DELL'ALTO ADIGE. By
Merio Toscano. [Storia e società.] (Bari: Editori Laterza. 1967. Pp. xxiii,
745. L. 7,500.)

PROFESSOR Toscano has earned a wide reputation in Italy and in Europe generally for his scholarly studies on foreign relations, international law, the history of treaties, and international diplomacy. Fortunately, his reputation has reached the United States, especially since the publication in English of his *The History of Treaties and International Politics* (1966).

The present highly controversial study is based on a course of lectures held during the academic year 1966–1967 at the Faculty of Political Science of the University of Rome. The author implies that he would not have published this study were it not for the fact that a huge volume by Karl Heinz Ritschel, *Diplomatie um Südtirol*, appeared in 1966.

Since Toscano's volume is based exclusively on diplomatic history and not general history, the author makes no reference to Mazzini's views that the frontier of Italy extends to the Brenner Pass; nor does he discuss Cesare Battisti's convictions on what constituted the northern frontier of Italy at the outbreak of the First World War. Instead, the study begins with a careful analysis of the early projects of the Italian Minister of Foreign Affairs, Marquis Antonino di Sangiuliano, and the terms under which Italy participated in the First World War; the study ends with Austria's partially negative reply of March 30, 1965, the global hypothesis discussed by the Ministers of Foreign Affairs of Italy and Austria in Paris on December 16, 1964, to implement the 1960–1961 resolutions of the United Nations in an effort to resolve the Italo-Austrian controversy.

Availing himself of unpublished documents that he was able to consult in Rome's Central State Archives, the Archives and Library of the Ministry of Foreign Affairs, the Berlin and Vienna archives, and in the private personal archives of De Gasperi, Toscano insists that Italy's demands on the Alto Adige in 1918 were based exclusively on strategic grounds and not on imperialistic aims. In support of his views, which he admits are entirely personal, Toscano stresses the illusions, delusions, and resentments of the Italians that resulted from the position taken by President Wilson in the discussions that preceded and followed the Paris Peace Conference. A careful examination of Italian archival documents permits Toscano to prove beyond the shadow of a doubt that Stresemann lied when he declared that Mussolini had requested him to guarantee the Brenner frontier. Furthermore, the conversations between Mussolini and the Austrian Chancellors Dollfuss and Schuschnigg reveal the persistent interest of Vienna in the Alto Adige and that, for the first time, Mussolini agreed to discuss the subject with a foreign power. Toscano rejects the view, expressed in recent German literature, that blames the Italians for the well-known 1939 agreement on options. During the 1946 conferences between De Gasperi and Grüber, the former did not conclude the agreement in order to save the Brenner frontier; rather, he did so only when he had been advised that it had previously been decided that the frontier with Austria would remain unchanged.

Toscano draws on his own personal experiences as a delegate to the General Assemblies of the United Nations from 1956 to 1966, and on his personal participation at several meetings held between the Foreign Ministers of Italy and Austria. Toscano is thus able to present a complete picture of the negotiations that took place in fulfillment of the resolutions of the United Nations of 1960 and 1961. Toscano arrives at three fundamental conclusions: that the Italian governments have consistently followed the line of conduct adopted since the first Italo-Austrian encounter in 1961; that in its desire to resolve the international controversy, Italy has been willing to adopt new measures destined to improve the status of the local populations; and that these measures and the position taken have been repeatedly approved by the Italian Parliament.

Toscano insists that at the basis of the entire controversy lies the fact that many officials at Innsbruck, Bolzano, and Vienna are still unconvinced that they should definitely renounce any claims on the Alto Adige. It is true, he admits, that errors have been made on both sides, errors that have served only to complicate the problem; nevertheless, the point of departure remains the same: nonacceptance of the Treaty of St. Germain, subscribed and ratified in 1919-1920 and confirmed in 1947 at the end of the Second World War and again confirmed by the State Treaty of May 15, 1955. Toscano expresses the hope that the time may soon come when there will be an honest and fruitful collaboration between the populations of the Alto Adige within the framework of European understanding which, without doubt, the Italians desire more than any other European people.

Columbia University

HOWARD R. MARRARO

ISTORIIA ODNOI KAPITULIATSII (KAK FRANTSIIA BYLA VYDANA GITLERU) [The History of a Capitulation (How France Was Betrayed to Hitler)]. By *L. P. Lavrov*. (Moscow: Izdatel'stvo "Mezhdunarodnye Otnosheniia." 1964. Pp. 335.)

SOVETSKO-FRANTSUZSKIE OTNOSHENIIA (1924-1945 GG.) [Soviet-French Relations (1924-1945)]. By *Iu. V. Borisov*. (Moscow: Izdatel'stvo "Mezhdunarodnye Otnosheniia." 1964. Pp. 550.)

No responsible scholar can read these two volumes without an overpowering sense of sadness. The problems faced by any historian when writing about his own times have been hideously multiplied for Soviet authors, who are betrayed by the sources denied them, the official points of view thrust upon them, and the incredible ignorance and prejudice with which they write.

Mr. Lavrov's book carries the imprint of the Institute of History of the Soviet Academy of Sciences and contains all the paraphernalia of scholarship, with a long international bibliography and many footnotes to French sources. It is, however, a tract designed to argue that "the reactionary ruling circles" of France surrendered that country to Germany in 1940, a position that is supported by much evidence, but that is presented here in a deliberately political although ostensibly scholarly form. Although no evidence is presented, the American role in 1938-1940 is made a particularly nefarious one, with the United States somehow or other assigned much of the responsibility for Munich. Lavrov's depth of understanding is, perhaps, best illustrated by his assertions that Léon Blum was an advocate of alliance with Hitler. The pact between Hitler and Stalin in August 1939 receives less than two paragraphs, and the closing of the French Communist press in that month is described in detail, with no mention of any relationship between that action and Soviet policies.

Lavrov was singularly unfortunate in the timing of the publication of his book—the summer of 1964—just three months before Khrushchev was ousted. Since Khrushchev was the only Soviet political leader, except Lenin, whose works were cited and whose speeches and papers were listed in the bibliography, the book must, therefore, have enjoyed the short life it deserved in the Soviet Union.

Borisov's volume, published by the Institute of International Relations, has the same fatal flaws. Borisov managed to write a book of 550 pages on relations between France and the Soviet Union from 1924 until 1945 without once mentioning the names of Stalin or Molotov. There is no mention of the Stalin-Hitler pact of 1939, an achievement as astonishing as the absence of any reference to those most responsible then for Soviet foreign policy. Soviet domestic history simply does not exist, and Borisov never refers to any of the momentous developments that occurred within the Soviet Union throughout that long period. The apparatus of scholarship is also displayed in this book, but even the Soviet reader can hardly have been misled by it. What a tragedy that such nonsense and poison should masquerade as scholarship!

Indiana University

ROBERT F. BYRNES

ENTMILITARISIERUNG UND WIEDERBEWAFFNUNG IN DEUTSCHLAND, 1943-1955: INTERNATIONALE AUSEINANDERSETZUNGEN UM DIE ROLLE DER DEUTSCHEN IN EUROPA. By *Gerhard Wettig*. [Schriften des Forschungsinstituts der Deutschen Gesellschaft für Auswärtige Politik E.V., Number 25.] (Munich: R. Oldenbourg Verlag. 1967. Pp. 683. DM 75.)

FOLLOWING upon some earlier treatments of the problem of the post-1945 *debellatio* and *rebellatio*, the disarmament and rearmament of the two Germanies, by Western writers, notably Jules Moch's *Histoire du réarmement allemand depuis 1950* (1965), Wettig's massive volume provides the Bonn version of this highly controversial topic. It is startling at times to discover how much of recent negotiations and discussions and their details, so often secret and secretive, should already be known so reliably. This still, however, leaves room for doubts in such matters as the precedence from time to time of diplomatic and military initiatives. To achieve this, the author has seen and used exhaustively not only the already considerable literature from both sides of the Atlantic, and this side of the Urals as well, but also such sources as the Stimson Papers. He has had the assistance of Bonn and several local governments, and he has employed excellent newspaper clipping archives with care and discernment. His work's forte is on intergovernmental negotiations and their details and the simultaneous handling of disarmament and rearmament on the two sides of the iron curtain, with Russia so much more forward and its former allies so hesitant in deciding to make military use of their respective occupation zones. It is weaker on the considerations, initiatives, and urgings of the military and the manifestations of public opinion on the controversial rearmament that in places, notably in France, went on for long periods and still continues in a blinding search for what amounts ultimately to *revanche* for the defeat of 1940 and for outdated forms of security including postwar controls over industry, police, and other armed and rearmable units. Such postwar demands have kept the new military security of the West forever below its achievable maximum from a time preceding even De Gaulle's manipulation of these sentiments to repudiation of the European Defense Community in 1955, the terminal point of this history.

Throughout the book, vast as it is, considerable tactfulness is observed. Thus, the demand for unconditional surrender is omitted altogether. And so are other tempting occasions for comment, such as the time when that supposedly strong citadel of capitalism, Alexander Hamilton's Treasury, actively supported proposals of permanently disarming Germany by uprooting capitalism at a critical moment. If this was called pastoralization, it meant eventual proletarianization, possibly Communism, for all Germany—an implication, again, ironically, first realized by the Department of War headed by Stimson, and then by Truman, who disowned Morgenthau.

Sherman, Connecticut

ALFRED VACTS

ENGLISH HISTORICAL DOCUMENTS. Volume V, 1485-1558. Edited by C. H. Williams. (New York: Oxford University Press. 1967. Pp. xvii, 1082. \$23.50.)

PROFESSOR Williams has divided this immense source book on the early Tudor period into six parts: "The Writing of History," "The Land" (travelers' accounts), "The Commonweal" (structure of society, social thought, and leaders of society), "Government and Administration," "Religion," "Daily Life in Town and Country" (social and economic problems). Though this division does not permit coverage of all subjects and such areas as foreign relations are omitted, within his scope Williams has selected documents to illustrate the various types of sources and has not left out anything of major import. None of his selections, however, are of independent value, and every document has appeared in print before. Nevertheless, Williams assembles and makes generally accessible much material.

His introductions to the various parts are not much more than descriptions of their subdivisions, and his "General Introduction" is such a general survey that it does not provide enough background for the nonspecialist to gain the maximum value from the documents. Some documents are prefaced by brief explanations, but there is nothing resembling the substantial commentaries of G. R. Elton's *The Tudor Constitution* (1960). Nor, it may be added, do any of Williams' introductions contain significant new interpretations.

His bibliographies (a "General Bibliography" and select bibliographies for each part) are more satisfactory as to content, and his comments on works are usually sound. Some notable omissions, however, include Elton's "Why the History of the Early-Tudor Council Remains Unwritten" (*Annali della Fondazione italiana per la storia amministrativa*, I [1964]), which would have enhanced the bibliography on the council, and a list of works on enclosure should certainly include Joan Thirsk's judicious and succinct *Tudor Enclosures* (1959). There is also a serious neglect in Williams' comments on works. While he gives due praise to the *Letters and Papers, Foreign and Domestic, of the Reign of Henry VIII* and most of the *Calendars*, he fails to warn students against the dangers involved in total reliance on those collections.

Criticisms notwithstanding, the fifth volume of *English Historical Documents* is a useful reference work that every early Tudor specialist should have. One would also like to say, with a reviewer of a previously published volume in the series, that it is "The kind of book that a student should himself possess for constant reference." The price, however, makes one wonder why Oxford University Press saw fit to quote that reviewer on the dust cover.

West Virginia University

MORTIMER LEVINE

THE AGRARIAN HISTORY OF ENGLAND AND WALES. Volume IV, 1500-1640. Edited by Joan Thirsk. (New York: Cambridge University Press. 1967. Pp. xl, 919. \$25.00.)

BECAUSE the late R. H. Tawney played so significant a role in planning *The Agrarian History of England and Wales*, it is appropriate that the inaugural

volume should cover the critical decades from 1500 to 1640. With the appearance of this work, a high standard in collaborative scholarship has been established. The regional approach has been followed, meaningless generalizations have been avoided, and all contributors have intelligently used local and central archives. Original chapters of monographic proportions by Joan Thirsk and Alan Everitt give this volume special importance.

Thirsk contributed three important chapters totaling two hundred pages: In Chapter i, "The Farming Regions of England," the conception of the uniformity of English farming is dismissed, and the author divides England into ten regions. The contrast between communities of mixed and pasture farming is drawn, and the effect of partible inheritance on landholding is evaluated. In Chapter iii, "Farming Techniques," Thirsk concludes that "improvements in arable and pastoral husbandry went hand in hand, each helping the other, and both serving to promote the specialization and interdependence of regions." She presents a balanced, regional account on "Enclosing and Engrossing" in Chapter iv. In less than sixty pages she has dissected arguments of the past fifty years and has brought forth new evidence. Some enclosure was not permanent, and beef and dairy cattle were probably significant in the system. After 1590 "profit margins no longer favored grass at the expense of grain." The "great revolution" wrought by enclosure was change in the way of life. Cooperative communal spirit was sacrificed to individualism.

Two chapters totaling about two hundred pages have been written by Everitt: Chapter vii is devoted to a study of farm laborers in which population, holdings, wealth, wages, and by-employments are discussed. The changing pattern of laboring life is emphasized, and the farm worker of the seventeenth century comes alive in this brilliant historical reconstruction. The farmer's life, his home, his aspirations, and the cleavage of laborers into forest and fielden ranks are discussed. Marketing of agricultural produce is the subject of the lengthy eighth chapter, which could stand alone as a monograph. Everitt's approach is regional, concentrating on specialization in agricultural products. Considerable attention is devoted to open markets, but private markets and marketing problems are analyzed. The second half of the period produced more economic, social, and organizational problems. Stuart regulative policies to assist the poor irked the private trader into political opposition to the crown.

Agricultural prices, farm profits and rents are analyzed by Peter Bowden in Chapter ix. Although Bowden has relied heavily upon the work of Rogers and Beveridge, there are some interesting, if not new, conclusions. The landlord was not so tightly squeezed during this period as others have suggested; population growth was a powerful influence on prices; the prices of agricultural commodities rose more than industrial or timber prices. In addition, differences in regional price and wage levels are emphasized.

Chapters x and xi discuss rural housing in England and Wales and contain descriptions, drawings, and photographs of houses of gentry, yeomen, husbandmen, domestic craftsmen, and village laborers. Perhaps the least original chapter is that devoted to landlords in England. Gordon Batho's portion on lay landlords is little more than a synthesis of recent work in administrative history.

Modern Europe

Joyce Young's section on ecclesiastical landlords is one of the few parts of the volume to deal with the period before 1540.

University of Richmond

JOHN F

THE KING'S GREAT MATTER: A STUDY OF ANGLO-PAPAL RELATIONS 1527-1534. By *Geoffrey de C. Parmiter*. ([New York:] Barnes and Noble, 1967. Pp. xiii, 322. \$11.00.)

THIS is not just another book on Henry VIII's divorce from Catherine of Aragon. Though Mr. Parmiter offers no new information, he has used the published sources so thoroughly that his book should serve as a reference work for students of the divorce. Indeed, the facts are presented so fully that the discerning reader will easily find evidence for arguments and interpretations different than Parmiter's, which are generally but not aggressively Catholic.

Parmiter is evasive on some important issues. He sets forth the usual explanations of the origin of the divorce—Wolsey's initiative, Henry's convenient conscience, his passion for Anne Boleyn, and his concern over the succession—but ventures no conclusion. He makes much of the so-called Spanish brief, a second dispensation of Julius II permitting Henry to marry his brother's widow, which practically destroyed the King's case based on the bull of dispensation. He skirts the crucial charge that the brief was spurious, however. And, curiously, he makes very little of the question of whether Catherine's first marriage was consummated, the issue that she herself evidently regarded as decisive.

Otherwise, Parmiter stands against Henry. He accepts the view that the very existence of a papal dispensation settled the matter of the validity of Henry's marriage for Catholics, the Levitical code notwithstanding, which may be deciding an early sixteenth-century problem by twentieth-century rules. While he justly condemns Henry's bullying of Clement VII, he fails to see that the Pope's deceptive promises gave the King ample cause to believe himself double-crossed. His conclusion that Clement had no alternative but to rule against Henry is correct according to his interpretation of the law, but he neglects to consider whether the Medici Pope would have found a way to grant the King his divorce if Charles V had not backed his Aunt Catherine.

Parmiter occasionally digresses to discuss the revolutionary changes in church and state in England that were by-products of the divorce. His treatment of these is generally sound but unoriginal. Following G. R. Elton, he acknowledges Thomas Cromwell as the man behind the changes. He is on shaky ground, however, when he maintains, solely on the unreliable testimony of a polemic of Reginald Pole, that Machiavelli's *Prince* was Cromwell's "political textbook." It cannot be proved that Cromwell ever read the work.

Criticisms and disagreements notwithstanding, *The King's Great Matter* is, if only for its detail and documentation, a useful addition to the literature of the divorce.

West Virginia University

MORTIMER LEVINE

A SHORT HISTORY OF THE UNIVERSITY OF EDINBURGH, 1556-1889. By *D. B. Horn*. (Edinburgh: University Press; distrib. by Aldine Publishing Company, Chicago. 1967. Pp. xii, 228. \$10.00.)

HISTORIES of universities and colleges have seldom become their subject. Who does not know the official history or the pious rhapsody, the dry bones of catalogues and board and faculty minutes, or the pulpy anecdotage of alumni, patched together with the ubiquitous "thus"? Both parade one puppet after another, many of them designated "great" (with a quaver), when truth to tell they were frauds. How often the reader learns everything but what is important. The authors report the founding of the law school, but fail to say in what respects university expansion reflected social and intellectual trends. Concentrating on the letter, they kill the spirit. A campaign "life" or an apology for General Pollutions, Inc. excites a sneer, but a comparable university history musters only a shrug.

To describe this first continuous narrative of the youngest Scottish university as a striking exception would be pleasant, for it is charmingly though too expensively presented, but the pleasure must be foregone. The account, useful as it is, lacks distinction. Mr. Horn has, it is true, been immune to piety and exaggeration. That Edinburgh reached superb heights in the eighteenth century no one can deny. William Robertson was its principal, and his scholarship combined with intellectual independence to create the atmosphere necessary to a first-rate university. Surely the author could also have made more of the university's relation to Scottish cultural history in other periods. Instead, we learn a little about this and a little about that; sometimes the pages are diverting, sometimes dull. Anyone who assumes, for instance, that the wilder shores of student misbehavior begin in Berkeley may discover what the sophisticated person has long appreciated: namely, that as in other respects the modern university is quite medieval. Anyone, furthermore, who assumes an orderly progression to maturity will quickly appreciate that the Topsy simile has no rival in describing the history of a university. For the rest, one may follow the struggles between town and gown, heightened by the proprietary sentiments of townsmen, the gradual growth of state supervision, recreation, university politics, sporadic campaigns for reform—constitutional, curricular, and administrative—enrollment "explosion," and admission of women. Perhaps his anxiety not to claim too much has impelled Horn to claim too little. Even his final sentence, "What can safely be said is that the University made a substantial and significant contribution to the educated manpower of the British empire in the later nineteenth century," is less a judgment than a stopping place.

University of Missouri

CHARLES F. MULLETT

THE RACE OF TIME: THREE LECTURES ON RENAISSANCE HISTORIOGRAPHY. By *Herschel Baker*. ([Toronto:] University of Toronto Press. 1967. Pp. 110. \$3.75.)

ELIZABETHANS were enamored of the past. Most of them read histories, and the range of material available to them was very wide, from broadsides through

the pocket chronicles of John Stow and the chronicle-histories of the playwrights on to the ponderous folios of Holinshed and Speed and the monographs of Selden. Few other subjects, perhaps only theology, interested so wide a social and economic spectrum of that time.

This phenomenon has attracted Professor Baker, and he has made it the business of his Alexander Lectures "to examine some of the anticipations and survivals in Renaissance historiography and to glance at some of their effects in literature." What he has given us is an admirable description of those ideas that all writers of history, good and bad, old-fashioned and newfangled, held in common. The first of his three lectures, on the truth of history, makes much of the Elizabethan passion for verity, but mentions as well the difficulties that had to be faced: the influence of religious controversy, the dangers of animadverting on contemporary politics, the problem of censorship. Similarly, the second lecture, on the use of history, exemplifies the Elizabethan insistence that the study of the past was a serious means of moral and political education for princes and ordinary citizens alike. Here, too, we are introduced to the Christian and cyclical views of the past, their interaction, and how they might be used to further the great didactic function of history.

Baker's first two lectures are excellent of their kind. Only when one comes to the third is it possible to quarrel somewhat with his interpretation. Up to this point, the analysis has not taken much account of changes; nor has it needed to. The third lecture, however, begins with a discussion of those dissatisfactions among Elizabethan historians that led to change and improvement; but instead of analyzing those changes, the importance of which he has maintained all along, Baker turns to a discussion of the increasing separation of history from poetry. While that is, indisputably, an important point, it should hardly supplant an appreciation of the great changes that took place in Tudor historiography, and, more important, of what caused them. Moreover, the book's insularity is surprising when one considers the author's previous work, but the fact remains that the effects of the Reformation on the Tudor historians are hardly mentioned; nor is the influence of the continental humanists—Machiavelli receives only a page. Yet these are three of the major forces transforming the medieval chronicle into the kind of history represented by Camden's *Britannia* and *Elizabeth*, by the monographs of Selden and Spelman, by Clarendon's account of the Revolution—all much praised by Baker. Thus we have, in *The Race of Time*, an urbane work, elegantly put together and elegantly produced, useful to those interested in the common beliefs of Elizabethan historians but disappointing to those who would like to know more about the forces that transformed the study of history during the century following the accession of Elizabeth.

University of Washington

F. J. LEVY

THE ELIZABETHAN MILITIA, 1558-1638. By *Lindsay Boynton*. [Studies in Political History.] (Toronto: University of Toronto Press. 1967. Pp. xvii, 334. \$9.00.)

THIS excellent volume is a careful study of the English militia's organization,

training, finances, weapons, and military potential from the accession of Elizabeth to the end of the personal rule of Charles I. The book's scope, however, extends much further. It traces the government's efforts to build the land defenses of the kingdom during a century when there was frequent fear of invasion. The government was compelled to throw the burden of payment upon the people; the cost of new weapons and of new methods of warfare was rising rapidly. Hence, there was a struggle between the pressures exerted by the crown and the canny evasiveness of the wealthier classes. It is not surprising that military efficiency waxed and waned with the acuteness of danger from abroad. Obligation to support the militia rested upon statutes of 1558 and upon royal prerogative as exercised through the office of lord lieutenant. But the methods of assessment were defective, and the statutes of 1558 contained vague sumptuary clauses: a man whose wife wore silk petticoats was considered wealthy enough to support a light horseman.

The wonder is that the government accomplished as much as it did. It increased assessments beyond the statutory level; it raised the standard of armor and weapons; it introduced training as distinct from inspection; it developed a warning system of pinnacles, posts, and beacons; it was able in 1588 to canalize large bodies of troops upon the major ports. Mr. Boynton's conclusion is that the Elizabethan militia deserves higher praise than has been accorded to it.

The militia degenerated in the early Stuart period. *Jacobus Pacificus* repealed the statutes of 1558. For some nine years the musters were perfunctory, so that, when the attempt was made after 1613 to revive the Elizabethan system, the militia displayed understandable signs of weakness. Ambitious plans of Charles I to increase its effectiveness proved abortive, and hostility to his government exalted assessments to the level of a constitutional grievance. By 1638 the militia was in decay.

There is little to criticize in this admirable volume except for a few devices to save space. Subheadings within chapters are listed in the table of contents, but are not repeated in the text. References in footnotes are often grouped in a way that defies immediate identification. But these are minor defects in an important work that illustrates how local history can be given national significance.

University of Minnesota

DAVID HARRIS WILLSON

THE ROYAL SOCIETY: CONCEPT AND CREATION. By *Margery Purver*.

With an introduction by *H. R. Trevor-Roper*. (Cambridge, Mass.: M. I. T. Press, Massachusetts Institute of Technology. 1967. Pp. xvii, 246. \$7.00.)

THE centrality of London's Royal Society during the flowering of the scientific revolution is generally acknowledged. Yet modern historical scholarship has only just begun to probe this important scientific organization. Margery Purver's recent book attempts to further this search.

The book is divided into two parts: The first contains Purver's interpretation of Francis Bacon's "New System" of sciences and the Royal Society's origin and role in carrying out Bacon's goals. The second includes her attempts to dispose of several previously suggested precursors of the organization.

The author's specific aim is to present an accurate genealogy and description

of the character of the society; her general goal is to vindicate man's conscious role in history by demonstrating that the scientific revolution was among "the great formative acts of the human intellect and will." It is argued that Bacon's inauguration of a "New System" of sciences was consciously continued by the Royal Society, which began creating "new sciences" for the benefit of man's estate. The group that formed the nucleus of the society was John Wilkins' Oxford club and not the utilitarian Londoners described by John Wallis, Hartlib's Puritan friends, or Gresham College. In this, Purver follows closely the history of Bishop Sprat.

Lurking behind the elaborate scholarly apparatus is a serious lack of sympathetic understanding of seventeenth-century science. Purver's distinction, for example, between Bacon as scientist and as philosopher is meaningless in its historical context. The author's claim, in justification of Bacon's anti-Copernicanism, that the Keplerian-Copernican universe was "not entirely correct" because the "sun is not . . . immovable but only appears so in relation to the solar system," is sheer nonsense. Similarly, to suggest that advances in molecular biology vindicate Bacon's linking of living and nonliving matter is to do violence to Bacon and to the philosophical temperament of his time.

The source of this misunderstanding is clear. Purver relies solely upon what was said *about* science and neglects the study of seventeenth-century science itself. It is not evident that she truly understands the character of the pursuit of science at the early Royal Society. Those "new sciences" said to have been created there (not a single new one is described or even named) would seem to require elaboration, historical development, and context.

Johns Hopkins University

ROBERT KARGON

ATOMISM IN ENGLAND FROM HARIOT TO NEWTON. By *Robert Hugh Kargon*. (New York: Oxford University Press. 1966. Pp. viii, 168. \$6.75.)

In his preface Kargon states his two objectives: first, to bring to the attention of historians the existence of two circles of natural philosophers who played an important role in the history of atomism; second, to trace the evolution of atomism and to illustrate the mechanism of its establishment in the seventeenth century. The first objective is fully achieved. Kargon demonstrates that a significant group, assembled by the patronage of the Earl of Northumberland and dominated intellectually by Thomas Harriot, embraced atomism at the beginning of the century. A generation later, a second group, partly derivative from the first, gathered around the Marquis of Newcastle, and atomism owes its effective establishment in England to the Newcastle circle. Although Harriot has been receiving increased attention of late and Hobbes, a key figure in the Newcastle circle, has always been well known, Kargon has brought to light a considerable body of unknown material that extends our knowledge of the early history of the mechanical philosophy.

I am unable to see that he enjoys equal success in dealing with his second objective. In some measure he does trace the evolution of atomism, but only in thumbnail sketches of complex systems that barely describe their most obvious

features. The fact that he still confuses the *vacuum coacervatum* with the *vacuum disseminatum* is perhaps an indication of how he has approached atomism, paying more attention to personal relations than to the system of thought. He consistently tends to conflate atomism with the mechanical philosophy, even though he is fully aware that the two are not identical. The first half of the book apparently seeks to explain the establishment of atomism in terms of personal contacts, and in the whole book next to nothing is said about what I take to be the crucial issue in its establishment: its advantages (real or imagined) as a philosophy of nature. Is it significant to know how an individual was made aware of atomism? For a scientist in the seventeenth century, we should need to ask instead how he was kept ignorant of it. The question is not how men became aware of atomism, but why they embraced it, and Kargon has said very little indeed that is addressed to the issue.

The final part of the book, devoted to Newton, builds its argument around the problem of hypothetical speculation and certainty in relation to the mechanical philosophy. In my opinion, Kargon has framed his discussion of this important problem in the wrong terms, suggesting in conclusion that Newton employed mathematical physics to raise the basic tenets of atomism from the level of speculation to the level of certainty. Since the citation from Newton at this point repeats Gassendi's argument from half a century earlier, I fail to see in what the increased certainty consists. Kargon's book contains important contributions to our knowledge of the seventeenth century. I feel compelled to add, however, that it also misses opportunities for still greater contributions.

Indiana University

RICHARD S. WESTFALL

THE RESTORED HOUSE OF LORDS. By *Maxwell P. Schoenfeld*. [Studies in European History, Number 9.] (The Hague: Mouton & Co. 1967. Pp. 244. 33 gls.)

IN this book Mr. Schoenfeld sets out to describe the process by which the House of Lords, formally abolished in 1649, successfully recovered its constitutional position and indeed went forward in the late seventeenth century to acquire even greater political influence than it had enjoyed in the years immediately before the Civil War. The book is almost strictly chronological in plan, moving forward from the decades before the war until the constitutional position had hardened and settled in 1662. Schoenfeld describes the abolition of the House of Lords and Cromwell's subsequent dilemma as he came to see the need for a second house; the collapse of his schemes and the full revival of the pre-1640 constitutional arrangements; and, finally, the day-to-day activities of the upper house in the Convention Parliament and in the first two years of the Cavalier Parliament, its influence upon legislation, and its relations with both Commons and crown.

Schoenfeld has largely confined himself to the well-known printed sources, and he covers much the same ground already traversed by Sir Charles Firth, A. S. Turberville, Sir Keith Feiling, and others. He does not appear to have any new insights or new interpretation to offer, and his main concern is to describe what happened rather than to explain why it happened as it did. He has some

welcome new statistical information about the peers in 1660 and about the membership of parliamentary committees, and he has performed a valuable service in collecting all the more or less familiar facts about the peerage at this period in one place. Schoenfeld has helped us a little way along the road toward understanding the reasons for this striking political recovery, toward assessing the over-all effect of the Interregnum on peerage status and finances, and toward knowing why the House of Lords adopted the positions it did about the various important pieces of legislation during the early years of the Restoration.

Princeton University

LAWRENCE STONE

THE FINANCIAL REVOLUTION IN ENGLAND: A STUDY IN THE DEVELOPMENT OF PUBLIC CREDIT, 1688-1756. By *P. G. M. Dickson*. (New York: St. Martin's Press. 1967. Pp. xix, 580. \$16.00.)

ONE of the most truly revolutionary aspects of the "revolutionary settlement" following 1688 was the creation of the national or parliamentary debt. Hitherto, state debt had been little more than the personal debt of the monarch, subject to the vagaries of his character, administrative ability, and longevity. Thus, lending to the crown was a speculative business in which successful plungers covered their risks by assuring themselves repayment relatively quickly through the assignment of the produce of specific branches of the revenue. Long-term lending to the government was virtually unknown. The great innovations of Godolphin and Halifax after 1688 were to provide parliamentary authority for borrowing and the statutory assignment of revenues for payment of interest or principal sufficient to assuage the doubts of the most timorous investor. So successful were these new arrangements that the long-term funded debt rose from nothing at the beginning of Godolphin's administration at the Treasury (1690) to over seventy million pounds at the death of Henry Pelham in 1754. In the same years, government short-term indebtedness changed from suspect and heavily discounted paper into the choice pickings that the Bank of England reserved for itself and other insiders. The rates of interest paid by the government declined from 7-14 per cent in the 1690's to 3-3½ per cent after Pelham's refunding of 1749-1752. While rustic moralists expressed shock at the seeming vast expansion of the moneyed *rentier* class, the economic growth of the country was undoubtedly aided by the creation of new highly liquid forms of investment that made much of the older hoarding by businessmen, trustees, and families unnecessary.

These striking developments are the subject of an admirable new work by P. G. M. Dickson that is far-ranging in its scholarship, perceptive in its questions, authoritative in its conclusions, one of the rare works of modern scholarship that can be compared to W. R. Scott's three magisterial volumes of 1912 and that can be described as "works of reference that should be in every library." Dickson's usefully detailed treatment of the long-term debt is particularly valuable and will undoubtedly be standard. (I found some of the sections on the short-term debt harder to follow.) While the treatment of the relatively well-known 1690's was a bit skimpy, the chapters on the South Sea Bubble were helpfully filled with much that is new. Dickson has also done a massive amount of new work on the debtholders, going far beyond the old controversy about what proportion of the

national debt was held by foreigners. Investors in the various types of government stock and in the shares of the three great moneyed companies are classified by size, residence, sex, and, in part, by ethnic origin. His evidence tends to show debt- and shareholding dominated by middle and larger holders resident in and around London: bourgeois *rentiers*, bankers, merchants, and so forth.

Despite its impressive scholarship and enormous value, the book falls just short of being definitive. One is surprised by the apparently limited use of the Treasury In-papers (T. 1) in the PRO and by some omissions in the bibliography, not to mention some trifling typographical errors. More important, Dickson only half escapes from the charge of writing what Lüthy has called "anonymous history." While Dickson improves upon Scott by giving us hundreds of names in his text and notes, he does not do very much with those names. If it was impossible to deal with the mass of fundholders, could not something more have been done with the leaders?

I also have some doubts about the way Dickson passes off the possibility that some of the nominal fundholders may actually have been acting for others. In the East India Company we know there were obvious reasons for putting shares in the names of straw men in order to multiply votes. Personally, I find suspiciously high the proportion of stock apparently held in London, primarily by merchants and bankers there. Some distant investors may have preferred to hold stock in their own names, and there may have been good legal reasons for trustees to register the names of beneficiaries; but what about speculators, or merchants putting out temporarily idle funds for correspondents? If a merchant normally used his own name when acting for a correspondent, why should he do otherwise when dealing in the public funds or company shares? If, as I suspect, this is still very much an open question, then Dickson's data on the proportion of the debt and company shares held in London must be used with extreme caution.

University of Michigan

JACOB M. PRICE

LAWYERS AND THE COURTS: A SOCIOLOGICAL STUDY OF THE ENGLISH LEGAL SYSTEM, 1750-1965. By *Brian Abel-Smith* and *Robert Stevens*, with the assistance of *Rosalind Brooke*. (Cambridge, Mass.: Harvard University Press. 1967. Pp. xiv, 504. \$11.00.)

THE authors have properly subtitled this work a "sociological study." Therein, I suspect, lies the main objection the historian will make to the book. As with most such studies, it contains the usual use of historical materials in a narrow, deterministic fashion to prove the points of argument the authors wish to make, the usual neglect, or dismissal, of contradictory evidence, and the usual style of academic sensationalism—in short, the usual approach of the sophisticated muckraker.

The authors dislike the law in England as it presently operates. They are irritated with its narrowness in both scope and approach, and they assign blame for this situation on the separation of the profession, the legal educational system, and the smugness of the professionals both on and off the bench. On reading this work one has the feeling, however, that Abel-Smith and Stevens first formed their general dislike, then searched for the reasons for the prevailing situation, and

only then searched for historical justifications within which to frame their conclusions. While this may be sound journalism, it is historically questionable.

This approach is reflected in a number of areas. One example is to be found in the conclusion that the present profession is nearly unreformable, and, at the same time, the authors dismiss the great nineteenth-century reforms of the profession with the most casual contempt. Further, the bench is blamed for having failed to make pronouncements on civil rights, but the authors neglect to point out that most of the substantive and procedural rights, with the exception of franchise, had been achieved, in form, in England in the century and a quarter before their study begins.

Specifically, the authors indict the English bench for having failed to pronounce public law in the sense of the American Supreme Court, and, further, they are critical of the profession for allowing far too much arbitration to fall into the hands of administrative tribunals. In answer to the first charge, it might be pointed out that the English Parliament, since the 1820's, has been largely reform-minded, while the American Congress, with notable exception, has not. Moreover, the American Supreme Court has usually entered into the area of public law with considerable hesitation and has suffered much abuse for such entry, as in the Court's recent decision to equalize the franchise. Rather than blaming the English courts for failing to pronounce such law, the authors should praise Parliament for relieving the profession from such contention. In the business of administrative tribunals, the authors make no attempt to prove that such tribunals have given injudicious, inadequate, or unappreciated aid at inappropriate costs or under undue delays; nor have they cited any great dissatisfaction on the part of the English citizenry with such tribunals. In view of all this, one cannot help being somewhat bewildered by the charge, and by the rancor with which it is stated.

In justice it must be noted, however, that this book emanates from a disciplinary posture that requires the type treatment of the subject that I find objectionable. Measured within the context of this posture, the book is a useful criticism of the English legal profession and system.

University of Colorado

WALTER G. SIMON

THE BRITISH EMPIRE BEFORE THE AMERICAN REVOLUTION. Volume XIII, THE TRIUMPHANT EMPIRE: Part I, THE EMPIRE BEYOND THE STORM, 1770-1776; Part II, A SUMMARY OF THE SERIES; Part III, HISTORIOGRAPHY. By *Lawrence Henry Gipson*. (New York: Alfred A. Knopf. 1967. Pp. xlii, 454, xxvi. \$10.00.)

WITH the publication of this thirteenth volume Professor Gipson brings his monumental history of *The British Empire before the American Revolution* to its conclusion. While a fourteenth and final volume is promised, it will be devoted to a comprehensive bibliography. Here the text is made complete. It is an occasion calling, above all, for congratulations to the author for an extraordinarily impressive achievement.

The preface informs us that the plan for the series was formulated in 1924. Publication began in 1936, when the Caxton Printers of Caldwell, Idaho, published the first three volumes. Over the course of thirty-one years the author

actually has seen through the press a total of sixteen volumes. Alfred Knopf assumed the responsibilities of publisher with Volume IV in 1939, and, Volume IX having been presented to the public in 1956, Gipson turned back to undertake a revision of the original three volumes. The first volume was reissued in the handsome design Knopf had given the series in 1958, and the remaining two appeared in 1960. That the author had not been thrown off stride by this interruption was indicated by the publication of Volume X in 1961 and of two more volumes in 1965.

Gipson has divided his grandly conceived history into four "books," as he has described them, each devoted to a subject of major significance. The first of these, entitled *The British Isles and the American Colonies* (Volumes I-III), provides a comprehensive and informative view of the "Old Empire" as it stood at mid-century, on the eve of the climactic Anglo-French struggle for imperial supremacy. In *Zones of International Friction* (Volumes IV-V) the stage is set for the conflict extending from 1754 to 1763 that the author described in the caption of his third "book" as *The Great War for the Empire* (Volumes VI-VIII). The fourth and final section of the history carries the title of *The Triumphant Empire*, and it is primarily concerned with the postwar crisis leading to the American Revolution.

In this last volume of the text Gipson turns his attention first to developments, falling mainly after 1770, in those parts of the Empire that did not separate from it in 1776. A general discussion of the intriguing question of why some colonies rebelled and others did not is saved for Part II, a summary of the history's main themes that hard-pressed graduate students through many years to come will find especially welcome. Part III departs from the usual pattern of historiographical essays to discuss individually the works of approximately sixty authors, British and American, who have written on the period. Some of them, Thomas Hutchinson, for example, were contemporary with the events of which they wrote, but most of them are later historians, including a number now active. Where Gipson disagrees, as more than once he does, he states his views with characteristic good humor and a generous acknowledgment of the value in approaches and interpretations other than his own.

It is in this same spirit that his own history should be, and undoubtedly will be, finally judged. He has written from a particular point of view, basically that of the so-called imperial school of historians led earlier in the century by George Louis Beer and Charles M. Andrews. In so doing he has significantly rounded out the work they began, and by the range and depth of his investigations he has answered many questions that will have continuing interest for all students of the period. Only a few of them will find the time to read the entire work, but they will find occasion to read in it again and again, and with profit.

Princeton University

WESLEY FRANK CRAVEN

THE PROBLEM OF BURKE'S POLITICAL PHILOSOPHY. By *Burleigh Taylor Wilkins*. (New York: Oxford University Press. 1967. Pp. viii, 262. \$7.70.)

THE original difficulty contributing to problems of Burke's political philosophy is

that he was not "a systematic thinker" or "a very precise philosopher." Yet he was coherent and consistent in spite of his reluctance "to go back always to first principles." Wilkins' emphasis upon the historical situation and avoidance of an "overinterpretation of the texts" helps him against difficulties Burke created.

The studies of Leo Strauss, Peter Stanlis, and Francis P. Canavan require the student of Burke's thought to confront the problem of the natural law. Wilkins finds Burke committed to the natural law, but he thinks that Strauss and Stanlis separated Locke too sharply from the classical-Christian tradition and Burke too sharply from Locke. At once the problem of natural rights appears, to be postponed until Wilkins has discussed Burke on human nature. Wilkins' analysis of Burke's *Inquiry into . . . the Sublime and Beautiful* is significant for its central concern with the relation between passion and reason. He concludes that the role of passion in Burke's thought has been overstated, and he qualifiedly classifies Burke as a rationalist both in aesthetics and politics.

The last section, two-fifths of the book, discusses natural rights, not only the problem of rights and duties, but such others as the relations among history, positive law, and natural law. Wilkins' awareness of the historical situation in which Burke found himself is most useful in this discussion. Wilkins seems momentarily to be arguing in a circle when discussing presumption and probability, but he extricates himself and in the end produces a satisfying analysis of what Burke meant by natural rights, civil rights, "real" rights, and historic rights. The conclusion is that "Burke's political philosophy was a conservative version of the natural law and not a denial of the natural law in the name of either history or utility."

This admirable book is good tempered, thoughtful, sensible, clear, and concise. Perhaps it is too concise, for the resolutions of certain problems are sometimes a bit summary. Wilkins owes a debt to Strauss and Stanlis; it is not easy to conceive of his book without theirs. Wilkins' moderation makes it likely that his statement of the theme of Burke's political philosophy will win preference over the earlier treatments of Burke and the natural law. His analysis of problems attendant upon Burke's commitment to the natural law is a considerable advance in the field of Burke studies, though it seems not to have taken account of anything published after April 1964.

University of Kentucky

CARL B. CONE

THE CORRESPONDENCE OF EDMUND BURKE. Volume VI, JULY 1789-DECEMBER 1791. Edited by *Alfred Cobban* and *Robert A. Smith*. (Chicago: University of Chicago Press. 1967. Pp. xxv, 494. \$13.50.)

IN their preface and introduction the editors say almost everything that a reviewer has to tell his readers about this volume. It covers a shorter period than any of the preceding five volumes in the Copeland *Correspondence*, but it ranges more widely because for the first time in his career as a public man Burke was deeply concerned with a great cause that was primarily European rather than British. Of the 225 letters in this volume, about half are new, and this same proportion of new letters is maintained for the 164 letters written in whole or in part by Burke and for the 47 letters to him. The *Correspondence* of 1844 prints only 31

letters for this same period. The letters tell of Burke's low state in the summer of 1789; of the continuing importance of the Hastings trial, apparently his sole remaining piece of public business; of his apparent indifference to the preliminaries of the French Revolution and his caution and slowness in reacting to it; of the importance of the activities of Dr. Richard Price and the Dissenters in shaping Burke's judgments about the possible impact of the Revolution upon England; of Burke's early decision that war and help from outside France would be necessary to the success of a counterrevolution; of the agonies the Revolution brought into the relations among Burke and his long-time political friends; and, as the period ended, of the resuscitation of Burke's reputation. In this period Burke's son assumed a prominence in his father's affairs that he had never before displayed.

It remains for a reviewer to say one thing the editors could not say. This volume is a superb work of discreet editorial scholarship, and, in part because of the greater difficulties encountered in editing it, the most striking of the six magnificent volumes to appear thus far in the definitive Copeland *Correspondence*. All the sadder, then, that on page 226 a letter "t" is attached to "the" rather than to the "went" that follows, and, because it seems always to happen this way, a reader guesses that when the volume arrived at the Sheffield workshop, it fell open to this page and the "typo" leaped up to the consternation of the scholars who had produced what otherwise is a perfect volume.

University of Kentucky

CARL B. CONE

THE RADICAL TRADITION: TOM PAINE TO LLOYD GEORGE. By
John W. Derry. (New York: St. Martin's Press. 1967. Pp. xi, 435. \$10.00.)

ONLY a rash historian would define the British radical tradition, and only an encyclopedic one would trace it from Lilburne and Colonel Rainborow, or even from Major Cartwright and Horne Tooke and Godwin, to the twentieth century. Dr. Derry is neither, and the main criticism of him, already made in several English reviews, is for choosing a title that promises much more than he intends to give. He does, it is true, discuss radicalism briefly in his introduction and his concluding pages. "It emphasises reason rather than reverence . . . it seeks to change the world . . . it is concerned with seizing the initiative, with moulding the future, rather than with responding to events . . . Its roots lie, not in the studies of the learned, but in the hopes of the self-educated and the dreams of the humble. It centres round the chapels of the north of England. . . . It reflects the ambitions of self-made men seeking recognition for their class. It expresses the protests of the outsider and the underprivileged." Elsewhere he calls it a middle-class movement with a moralistic strain and an expression of provincial discontent, and he notes its recurrent rural associations. By a study of representatives of different facets he hopes to discover its major elements.

The result is ten substantial essays, each one thoughtful and useful for the many quotations it contains. While its value for the student seeking short cuts is obvious, the difficulty remains. You cannot establish a tradition by a series of independent studies, particularly when the choice of subjects seems arbitrary or at least highly personal. Here is no patient tracing of thought and action, from

pamphlet to newssheet, from speech to association, from satire to cartoon, from election meetings to Parliament and ministries. Hume and Roebuck are missing, as are scores of other agitators or politicians. And those chosen can only be sketched in broad outline, with more attention to their speeches and writings than to their achievements. Generalizations are insecurely based: Paine's career "marked the entry of the common man into politics"; Cobbett was "the Gold-water of the early nineteenth century." Bentham (a radical?) is sympathetically treated, but scant justice is done to the fertility of his ideas concerning the machinery of government. John Stuart Mill (another radical?) is drawn without benefit of Joseph Hamburger's studies. There is a chapter on the Chartists, and another on Cobden and Bright. The other radicals are Robert Owen, Lord Randolph Churchill, Joseph Chamberlain, and Lloyd George. Derry is kind to Chamberlain, perhaps overkind, as in his brief reference to his dealings with Parnell and O'Shea in 1885. His essay on Lloyd George captures much of the man with discernment and discrimination.

University College of North Wales

C. L. MOWAT

ROMILLY'S CAMBRIDGE DIARY, 1832-42: SELECTED PASSAGES FROM THE DIARY OF THE REV. JOSEPH ROMILLY, FELLOW OF TRINITY COLLEGE AND REGISTRARY OF THE UNIVERSITY OF CAMBRIDGE. Chosen, introduced and annotated by J. P. T. Bury. (New York: Cambridge University Press. 1967. Pp. xv, 259. \$12.50.)

FROM 1832 to 1861 Joseph Romilly, nephew of Sir Samuel Romilly, the Whig law reformer, was registry of Cambridge University. He kept a diary from 1818, three years after he had become a fellow of Trinity College. This present volume, well edited and well printed, ends with the year 1842, when a deputation from Cambridge visited the Queen to congratulate her on the birth of the Prince of Wales. Undergraduate behavior was subject to criticism even then. "I hear that some of our men clambered on the chairs &c &c that one glass door was broken:—this is unfortunate and discreditable to us." There had been many earlier causes of complaint. Thus, at Bachelors' Committee in 1835 there had been "prodigious bad taste in the galleries . . . the youths imitated the usual Oxford proceedings: they gave three shouts for the Ministry, the Duke of Wellington &c &c . . . and three groans for Potter (S^rPr^r) Evans (S^r Mod^r) &c."

In general, the *Diary* is a readable and lively source rather than a basic document in university history. Most of the entries are too brief to explore critical issues or to illuminate controversial personalities. Thus, for example, though there is information about the election of William Whewell to the mastership of Trinity in 1841, there is virtually nothing about his ways of arguing or working. We have only one evaluation: "What a wonderful man he is! there is such a feeling of doing good in all his speculations." Far too many entries take the important things for granted: "Dined with Cummings (and others) . . . Enjoyed it very much, though Clark and Hopkins preached over" their wine about the Reform Bill of 1832. What they preached we shall never know.

The most incisive comments in the *Diary* relate not to politics but to religion. Romilly was an exacting critic of the hundreds of sermons that he heard, not all

of them in the course of his formal duties. It is not easy, however, to generalize about his religious attitudes. As a bachelor clergyman he was urbane, not eccentric, and tolerant, not profound. He was opposed to religious tests, said nothing about Newman or Pusey, objected to one attack he heard on "the Evangelical party" (his only reference to this group), and mentioned, without comment, a visit of one of his colleagues to Leeds where he witnessed "the Anglo-Catholic magnificence of the service at Dr Hooks—5 offic^g ministers at the Altar, litany &c creeds chanted, anthem sung, preaching in Surplice &c." The "&c's" multiply. This is a *Diary* without revelations, more valuable for its flavor than its content.

University of Sussex

ASA BRIGGS

THE DEVIL DRIVES: A LIFE OF SIR RICHARD BURTON. By *Fawn M. Brodie*. (New York: W. W. Norton and Company. 1967. Pp. 390. \$6.95.)

RICHARD F. BURTON (1821-1890) was a perplexing combination of swordsman and poet, soldier and linguist, explorer and translator, "naughty Frenchman" and dignified consul, Arab sheik and devoted husband. Born a century too soon, he was a rebel in Victorian society. He published forty-three volumes of exploration and travel, two of poetry, and over a hundred articles. He also translated sixteen volumes of the *Arabian Nights*, six of Portuguese literature, two of Latin poetry, and four of folklore. At least six of his volumes are still classified as erotic.

Mrs. Brodie has taken as title for her third biography Burton's own answer as to why he explored in Africa: "The Devil drives!" In her psychological study, however, she accepts this as only a partial explanation of her subject's motivation. Burton, she asserts, was fascinated by all things devilish and prided himself on looking like Satan. But, like other great explorers, he went into Africa to seek solace from the death of loved ones and to "return, if not all the way back to the cradle, at least to childhood." His passion for the forbidden she attributes mainly to his mother and wife, both of whom subtly encouraged his wildness and adventurousness while repudiating his sexuality. Through most of his life, she states, he was seeking to resolve an unfulfilled sexuality. The result was a complicated, anguished, and perplexed personality engaged in a desperate quest for identity. To the author, Burton was driven not by the Devil, but by his wife Isabel, the epitome of Victorian prudishness, who curbed his finest ambitions and burned his culminative and probably greatest manuscript, the *Scented Garden*, and most of his journals and diary.

This book is by far the most interesting and informative of the eleven biographies of Burton. Based on extensive research, it is amply, though not fully, documented, and it sparkles with quotations and lively narration. It also corrects many misconceptions fostered by other biographers. It is, however, not without fault. The book deserves more maps, and the narrative at times betrays the author's lack of depth in history, geography, and anthropology. It creates the impression that too much emphasis was given to the sensational, especially the sensual, aspects of Burton's life. Its greatest weakness is, ironically, that which purports to be the biography's greatest strength—the analyses of Burton's motiva-

tion—but for this the author, unable to bridge the gap of sex and understand her subject as only a man could, should perhaps not be criticized.

Syracuse University

ROBERT G. GREGORY

VICTORIAN DUKE: THE LIFE OF HUGH LUPUS GROSVENOR, FIRST DUKE OF WESTMINSTER. By *Gervas Huxley*. (New York: Oxford University Press. 1967. Pp. xiii, 214. \$8.80.)

IN 1874, when with Gladstone's "dying breath" the richest man in England became the first duke of his line, the earliest peerage in the ancient Grosvenor family lay little more than a hundred years back. Inheritance, marriage, and management, together with the growth of London, had created the conditions for a perfect Victorian flowering. Mr. Huxley, who in his *Lady Elizabeth and the Grosvenors* (1965) provided convenient background information on the family and its fortunes before the nineteenth century, has written a careful, factual, unpadding biography solidly based on the unpublished Grosvenor archives. The book is biography, not history, and some scholars will regret that the development and management of the London estate are so lightly sketched and the Duke's racing interests so fully; thirty-seven of his horses are named in the index. But Huxley has done what he intended, and has done it well.

The life of Hugh Grosvenor (1825-1899) was an ample one. Everything his rank and resources called for he undertook and achieved successfully, from the day he was a page at Queen Victoria's coronation to his part as lord lieutenant of London at her Diamond Jubilee. As parent and grandparent, landlord and neighbor, man of business with great estate responsibilities and building enterprises, sportsman and stud owner, unostentatious philanthropist and host at Grosvenor House with its indoor staff of twenty, he was the ideal duke. Politics too formed part of the duties of his station. He described himself as "a Whig, in favor of free trade, will support principles of rational progress tempered with moderation." Though he twice broke with Gladstone (in 1866 and 1886), there was no personal rift, and he remained a Gladstonian in his concern for moral questions and quite unconcerned about party as such. All in all, Huxley has given us a credible picture of unrelieved Victorian virtue, which, if a little dull, is no fault of his. Though possibly he has muted some less happy aspects of the Duke's life, there is no odor of undisclosed skeletons, Freudian or otherwise. Family portraits and buildings are illustrated, and there is a chart of the Duke's descendants.

Clark University

H. DONALDSON JORDAN

1867: DISRAELI, GLADSTONE AND REVOLUTION. THE PASSING OF THE SECOND REFORM BILL. By *Maurice Cowling*. [Cambridge Studies in the History and Theory of Politics.] (New York: Cambridge University Press. 1967. Pp. ix, 450. \$13.50.)

THE English Reform Act of 1867 had a more devious and complicated history than any other important piece of legislation of the century, and Mr. Cowling has sought to reconstruct that history in all its deviousness and complexity. His work

is a major contribution to Victorian scholarship and an admirable exemplar of the thesis that events can only be understood in the context of their minutiae. We cannot too often be reminded of the loss of truth and perversion of meaning that inevitably accompany the equally inevitable selections and generalizations of the historian. It is a tribute to Cowling that he induces in us a desire for even more complexity—or perhaps an order of complexity that lies outside his self-imposed boundaries.

Cowling's contention is that the Reform Act can be entirely comprehended in the "process of decision-making," the "politics of continuous tension," out of which it arose. Recounting this political process in remarkable detail and with compendious documentation, he finds little or no evidence of the practical effect of forces, powers, influences, motives, ideas, or attitudes other than those of a most conventionally, narrowly conceived "political" kind. He finds, for example, no effective causal relationship between social or industrial developments and this particular political development, between popular movements and parliamentary intrigues, between ideology or attitudes and the impulse to reform. Nor does he find any differentiation of parties, whether in principle, historical commitment, habit, prejudice, or social composition. Nor does he find any effective differentiation of persons in these same terms. The only differentiations of person or party, the only tensions he perceives, are those created by the exigencies of day-by-day politics. Successful politicians, he writes, "cannot usefully be said themselves to have wanted, desired, or believed anything except what was wanted by all other participants in the system." And that "system," as he conceives it, is totally self-contained and self-defining.

Cowling's narrative of the events of 1865–1867 entirely supports his conclusions—for good reason. It is based upon a formidable mass of primary materials, largely private letters. One must admire and be grateful for the industry that went into the assembling of these materials and the resourcefulness that made of them a sustained and intelligible narrative. Yet our admiration and gratitude must be tempered with caution. For his sources have obviously, as Cowling is well aware, predetermined his conclusions. He has deliberately chosen to rely on letters written exclusively at the moment of action rather than parliamentary debates or memoirs, because he is only willing to credit what is immediate, personal, explicit, and observable. It would be fatuous to repeat here the familiar arguments against Namierism, behaviorism, or Positivism; Cowling knows perfectly well what he is doing and does it with uncommon intelligence and verve. The assumptions and methods of this book are as overt as his conclusions, and they are identical with those informing his earlier books on Mill and on political science. Other historians dealing with the same subject will have to take serious account of these assumptions and methods, as they will have to take account of the wealth of raw materials he has so generously supplied. But they need not feel bound by those assumptions and methods or limited to those materials. They might even be tempted to inquire further into the subject of Cowling's title—a title peculiarly inapt for a book that sees no substantive difference between Disraeli and Gladstone and concerns itself not at all with the question of "revolution" (except, by implication, to deny meaning to it).

Brooklyn College

GERTRUDE HIMMELFARB

INDUSTRIAL RETARDATION IN BRITAIN, 1880-1914. By *A. L. Levine*. (New York: Basic Books. 1967. Pp. ix, 201. \$6.95.)

THIS brief but important monograph, the outgrowth of a doctoral dissertation at the University of London, provides authoritative answers to two basic questions that have long intrigued students of modern British economic history: What was the quality of Britain's industrial performance during the decades before 1914, "using contemporary manufacturing practice in the United States and Germany as the norms"? Secondly, and more importantly, what were the social and economic factors responsible for the observed relative inferiority ("retardation") of Britain? The information is derived mainly from governmental and trade-union reports, articles in economic and technical journals, and other publications of qualified contemporary observers.

The author devotes the first part of his study to a discussion of major innovations during the second Industrial Revolution. Taking productivity as his criterion, he validates his hypothesis that British manufacturing industries after 1880 generally fell behind the industries of the United States and Germany in both technology and organization. In Part II he analyzes in detail the nature of the British technological and organizational shortcomings. In Part III, the longest and most valuable section of this volume, Levine critically appraises the complaints directed by previous writers against the quality of British industrial entrepreneurship and against the attitude of the trade-unions and the working classes generally toward mechanization and other management innovations. In similar critical fashion, he also analyzes the large variety of more narrowly economic reasons that have been suggested to explain Britain's relative retardation. Levine winds up this excellent and thought-provoking study with a brief chapter succinctly summarizing his own conclusions: "... Technical and organizational lag in British industry was, more than anything else, a question of entrepreneurial responses—responses that drew their character from certain social, and social-psychological circumstances, but which were further conditioned by the economic (and other) consequences of Britain's earlier start as an industrial power, of the nature of the market for British goods, of the mix of the British export bill, and of the rate of growth of Britain's national product (with the last being possibly the most important of these)." This last, as he shows, was a causal factor, as well as an effect. Although uncertain about the retardative effects of trade-union job and income protective policies, he concedes that such policies, where successful, diminished the incentive for employers to innovate.

Rutgers University, Newark

SYDNEY H. ZEBEL

WINDS OF CHANGE, 1914-1939. By *Harold Macmillan*. (New York: Harper and Row. 1966. Pp. vi, 584. \$10.00.)

THIS first volume of Harold Macmillan's autobiography, which carries his life and times down to the outbreak of war in 1939, is very disappointing. His account of his childhood, adolescence, and young manhood is too careful of himself and too polite to those around him. He is only inadvertently revealing when he lamely tries to explain that he did not have all the advantages of circumstance and privilege, and especially when he writes about that remarkable

woman, his mother. It is indeed difficult to arouse sympathy for one who had been born to the comfortable and well-connected publishing house of Macmillan, educated at Eton and by private tutors, attended Oxford, commissioned during the Great War in the Grenadier Guards, served as ADC to the Duke of Devonshire when the latter was governor-general of Canada, married one of the Duke's daughters, and was elected to Parliament as a Conservative in 1924 at the age of thirty. All these advantages were, moreover, reinforced at every turn by a dynamic and resourceful mother who did not scruple to use either her connections or influence in the interest of her son.

The carefulness, politeness, and basic reticence become even more evident when Macmillan turns to the heart of the matter in this volume—the proving of his political manhood. The story of his parliamentary career as a troublesome Tory backbencher between 1924 and 1939, very creditable to him indeed, is told in a curiously detached way. Representing, except for a brief interlude (1929–1931), Stockton on Tees, an industrial and largely working-class constituency, he was naturally most concerned with economic problems, particularly unemployment. His growing disillusionment with the Conservative leadership, however, had less ultimately to do with domestic issues than with foreign affairs. A warm admirer of Churchill, and a warmer admirer of Eden, Macmillan manfully resigned the party Whip after voting against the government in the Abyssinian crisis in 1936, and, though he chose to receive the Whip again on Baldwin's retirement, he was one of those within the party who actively opposed Chamberlain's policy of appeasement.

The story as told by Macmillan, however, does not grow richer with the retelling because it is lacking in those personal and analytical insights that might naturally be expected from one of the most astute, shrewd, and successful connoisseurs of political power of his generation. There are no intimate insights because Macmillan seems to have been a political lone wolf and did not exchange fundamental confidences. There is also very little real analysis because Macmillan is more interested in effect than in cause, and he finds his strength in narrative rather than in plot. While these faults obviously do not militate against the making of a successful political career, they seem to be disastrous for the writing of memoirs.

University of Chicago

EMMET LARKIN

INTELLIGENCE NOTES, 1913–16, PRESERVED IN THE STATE PAPER OFFICE. Edited by *Breandán Mac Giolla Choille*. [Chief Secretary's Office, Dublin Castle.] (Dublin: Oifig an tSoláthair. 1966. Pp. xl, 310. 55s.)

THESE Intelligence Notes have been chosen from the State Papers in Dublin Castle; their publication is part of the Irish government's contribution to the celebration in Ireland of the fiftieth anniversary of the Easter Rising. Derived from reports sent to the Crime Branch Special of the Chief Secretary's Office by the inspector general of the Royal Irish Constabulary and by other police officers, these Intelligence Notes were then prepared annually by the Judicial Division of the Chief Secretary's Office for his use. The situation in Ireland for each of these years, county by county, is reported. We learn about public reaction

to Home Rule, about the Ulster crisis, about lingering rural disorder, about labor troubles in Dublin, about the activities of clubs and societies, to name a few subjects; now and again there are glimpses of local and personal matters that the social historian should find interesting. For 1916, the notes confirm the widely held view that there was little sympathy throughout the country for the rebels of the Easter Rising. It was the post-Rising policies of the British government that pushed Irish opinion in a more radical direction. The volume's introduction discusses some of the historical background of intelligence work, the methods used, and the administrative departments involved. Needless to say, these notes do not exhaust the original sources for these crucial years in recent Irish history. The Military History Bureau, for example, has recorded the reminiscences of participants, and some work of this nature was also carried on by Radio Telefis Eireann during 1966. There have been many books in recent years on the Easter Rising. Perhaps someone will follow up the editor's suggestion and compile a bibliography of works on the Rising and the events leading to it.

Connecticut College

HELEN F. MULVEY

LEADERS AND MEN OF THE EASTER RISING: DUBLIN 1916. Edited by F. X. Martin, O. S. A. (Ithaca, N. Y.: Cornell University Press. 1967. Pp. xii, 276. \$5.95.)

THE Easter Rising puzzled its own and later generations. It ran counter to the sentiments of the majority of the Irish; it marked a return to physical force after a century of parliamentary activity; it demanded separation at a time when Home Rule seemed inevitable. It was planned, ineptly, by men who were relatively unknown; the limited amount of success possible was destroyed by inflexibility and by a secrecy that kept leaders and followers ignorant of the proposed rebellion. Despite all this, its influence was enormous; the leaders and the Rising quickly became legendary.

This series of essays attempts to explain how and why the Rising happened, to set it in the context of the Ireland of its time. There is little on the rebellion itself. Its special quality is faced only by the men of 1916: by Edward MacLysaght in his "Larkin, Connolly, and the Labour Movement," by Florence O'Donoghue in his "Ceannt, Devoy, O'Rahilly and the Military Plan." The role of Ulster as initiating the return to physical force receives considerable attention in the accounts of Craig, Carson, MacNeill, Republican Ulster, and Casement. The Fenian tradition of force is stressed in contributions on the Irish Republican Brotherhood, especially on the men who signed the Proclamation of the Republic. There is less interest in the activities of the Citizen Army and the Clan na Gael. Separatism is considered in essays on Sinn Féin, on Irish Ireland and the Gaelic League, and in the emphasis placed on the part the Gaelic revival played in attracting poets to politics. Official policy is covered in sections on Birrell, Nathan, Wimborne, and General Maxwell. The impact of the rebellion is viewed in its effect on the Irish Parliamentary party, on women, on the Roman Catholic clergy.

Leaders and Men originated as nineteen talks on Radio Telefis Eireann. Each contribution is self-contained; consequently, the series abounds in repetitions and inconsistencies. Principal sources are given, but not footnotes. In general, the

essays are cool, but not detached; no embarrassing questions are asked, and no unpleasant assessments sound discordant notes. The volume is useful as a supplement to accounts of the Rising. It avoids the how and simplifies the why, but it does provide a good example of various forces at work in the Ireland of the time.

San Jose State College

MARY D. CONDON

SOUTH TO THE POLE: THE EARLY HISTORY OF THE ROSS SEA SECTOR, ANTARCTICA. By *L. B. Quatermain*. (New York: Oxford University Press. 1967. Pp. xx, 481. \$12.00.)

THE Ross Sea Sector is historically the most interesting of the several pie wedges into which the Antarctic is now divided. Here is where the great James Clark Ross penetrated farther south than anyone up to his time and named smoking Mt. Erebus and dead Mt. Terror after his ships. Here is the icy wilderness of Shackleton, Scott, and Amundsen. These names conjure up pictures of courage, hardship, and fortitude familiar to everyone interested in polar exploration before the days of Admiral Byrd.

Quatermain, himself no stranger to the Antarctic, has written a fine history of the early period of exploration in this sector. After some appropriate introductory remarks, there are detailed accounts of the expeditions of Ross; Borchgrevink in the *Southern Cross*; Scott's first in *Discovery* with its great sledging journeys; Shackleton, with little *Nimrod*, during which he sledged to within ninety-seven miles of the Pole, and three of his companions reached the South Magnetic Pole; Scott's tragic finale with *Terra Nova*; Amundsen's triumph; Shackleton's final debacle; and various lesser efforts.

The story is gripping; the very monotony of the sledge trips, the endless ice, the interminable dull rations, and the constant falling of men, ponies, and dogs into awesome crevasses, create a hypnotic fascination. How, one finally wants to scream, could men voluntarily drive themselves into such misery. But drive themselves they did, and in doing so each man contributed to the knowledge that was slowly accumulating about the forbidding Antarctic wasteland.

For decades writers have debated the merits, personalities, and motives of Scott and Amundsen. The latter usually comes out as an unscrupulous opportunist and the former as a generous and noble man. Reading the detailed factual descriptions of their expeditions gives one a somewhat different picture. Amundsen is still an opportunist, but he is also by far the more skilled in polar conditions, the better planner, the more intelligently cautious. Scott took chances on more than one occasion with his own life and those of his companions. He seldom left any margin for safety, and this, with his unreasoning prejudice against dogs, jeopardized his polar sledging journey from the start.

One lack that the reader feels in this excellent book is the absence of sketch maps in the text on a larger scale than the two maps and end papers provide, to show the tracks in detail of at least the more important sledging journeys.

Peabody Museum of Salem

ERNEST S. DODGE

THE THEATRE OF THE FRENCH REVOLUTION. By *Marvin Carlson*.
(Ithaca, N. Y.: Cornell University Press. 1966. Pp. xii, 328. \$10.00.)

ONCE actively studied in this country and Europe, the theater of the Revolution has been relatively neglected lately by historians. This well-written, clearly organized, and dramatically narrated account of the actors, playwrights, and producers of Paris between 1789 and 1799 should stimulate renewed historical research and analysis on this significant subject. The author, assistant professor of speech and drama and director of the university theater at Cornell University, writes for the general public rather than for the professional historian. Nevertheless, the latter will find Carlson's keen professional insights, dramatic flair, and literary skill rewarding. This vivid chronicle of the various Paris theaters is divided into ten chronological periods. Carlson also describes and illustrates those innovations in costume, acting, *mis en scène*, architecture, decor, organization, enterprise, and genres that make this period significant in the history of the theater. Despite his title, he deals only with Paris.

Carlson's basic thesis is that there was an essential interaction between theatrical production and revolutionary activity. The Paris theater *was* profoundly affected by contemporary social movements and by shifts in political power and opinion, but not always as described here. The Goncourts (1854) and Thiers (1821-27) upon whom he depends are inadequate guides to this period's social and political history. For example, his "Jacobins" and "sans-culottes" bear little resemblance to those revealed by Brinton's and Soboul's researches.

For developments in the theater Carlson bases his account, in part, on narrative sources. Although he recognizes that contemporary memoirs are highly biased, he sometimes cites them uncritically in contexts that may mislead the reader. His summaries of the plays are based on original texts, largely drawn from the rich White Collection at Cornell.

Mentioned but not developed here are the interrelationship of the theater and revolutionary festivals and the effective use of the theater as propaganda in France and elsewhere. One is reminded of the important contributions of Hyslop, Stewart, and Godechot not cited by Carlson.

Despite Carlson's self-imposed limitations (and certain factual errors) historians may recommend this interesting book to their students, provided it is read in conjunction with Hampson's *Social History of the French Revolution* and a good, up-to-date political history.

University of Kentucky

DAVID L. DOWD

ŒUVRES DE MAXIMILIEN ROBESPIERRE. Volume X, DISCOURS. 5th Part, 27 JUILLET 1793-27 JUILLET 1794. Prepared under the direction of *Marc Bouloiseau* and *Albert Soboul*. [Publication de la VI^e Section de l'École Pratique des Hautes Études (Sciences économiques et sociales) et de la Société des Études Robespierriistes.] (Paris: Presses Universitaires de France. 1967. Pp. 655.)

HISTORIANS have shared in French economic gains of the last two decades, in that many projects for publication of source materials, slowed down for years to

a snail's pace or in the case of the *Archives parlementaires* apparently abandoned unfinished in 1913, have taken on new life. This volume marks the completion of the important volumes of the complete works of Robespierre sponsored by the *Société des Études Robespierriennes*, which are devoted to his speeches. Marc Bouloiseau and Albert Soboul have done a superb, scholarly job of editing, aided by ten assistants whose names are duly recorded in the volume. The labor of comparing and collating the many different newspaper accounts of what went on in the Convention and in that remarkable sounding board (to put it mildly) for Robespierre, the Paris Jacobin Club, was enormous.

For, as is well known to all workers in the field, there was no single complete official report of what was said in the Convention or in the Jacobin Club. The *Moniteur* and the later volumes of the *Archives parlementaires*, put together with increasingly better scholarship from its start in 1879, served very well, but they were not the equivalent of the official stenographic reports that were to become a normal part of nineteenth-century parliamentary governments. It can be said that, as far as Robespierre's share of the proceedings in both the Convention and the Jacobin Club, this final volume comes as close as is humanly possible to what a *Journal officiel* would have done for Robespierre's speeches. The accounts in the *Moniteur* and in Aulard's *La Société des Jacobins*, the texts of the published speeches, and the surviving manuscripts of Robespierre have been carefully collated, variants noted, and all supplemented by accounts from newspapers, some of them rare indeed. The whole *apparatus criticus* is abundant, but not unnecessarily so. The volume is a great credit to contemporary French scholarship.

The work now completed will be indispensable for students not merely of the French Revolution but of the phenomenon of revolution in the modern world, or, as Professor James Meisel, not discouraged by the coining of a word like "Kremlinologist" dares to call it—"revolutionology."

The great speeches, carefully written out, of course, by Robespierre, did not need as much editing as his day-to-day interventions in debate. Still, it is most useful to have even the slight variations in the texts of such important speeches as that of 5 *Nivôse* of the Year II on the principles of the revolutionary government, that of 17 *Pluviôse* of the Year II on the principles of political morality that should guide the Convention in the domestic affairs of the republic, and, of course, the speech of 8 *Thermidor* that sealed his fate (this last does benefit by careful editing and much new material). The pieced together accounts of the session of 9-10 *Thermidor* are, considering the fact that they are mere source material, remarkably finished dramatic history. I should not, perhaps, abuse the privileges of the reviewer by putting in a bid for the comparative historical study of revolutions. Still, I find it hard to believe that even the most vigorous opponent of such study could read this volume without letting his mind at least hover a bit—and by no means unprofitably—over what has happened in our world in our time.

Harvard University

CRANE BRINTON

THE SOCIETY OF ARCUEIL: A VIEW OF FRENCH SCIENCE AT THE TIME OF NAPOLEON I. By *Maurice Crosland*. (Cambridge, Mass.: Harvard University Press. 1967. Pp. xx, 514. \$15.00.)

In this book, the author, lecturer in the history of science at Leeds, discusses certain aspects, mostly personal, of scientific activity in Paris, 1795-1825, stressing the roles played by Berthollet and Laplace, owners of substantial houses at suburban Arcueil. At first linked with the policies of Bonaparte, with the passing of years and the Empire the group of scientists became an establishment in its own right, determining future directions of science in France as well as the individual fortunes of younger scientists, French and foreign.

Dr. Crosland's task has been complicated by problems of arranging his material. The background, Napoleon's interested patronage of science, the state of the sciences themselves, the development of the *Académie des Sciences* into a section of the *Institut de France*, the personal relationships lingering from the past, all this takes much space, and not before we reach Chapter v do we visualize what is referred to as the "Society of Arcueil." Since it was not an academy of the type familiar in the Renaissance and the seventeenth century, its leaders wisely retained a purely social and personal status, with the ability not only to direct their own investigations, but probably also to avoid police and governmental interference. Turbulent times and rapid changes in regime between 1789 and 1804 made an independent haven for science extremely desirable. Berthollet's large house offered a laboratory, peace for experimentation, and opportunity for discussion and meditation, without contact with political intrigue or suspicion of subversion.

The society itself defies description in the usual terms: it had no constitution or legal standing, kept no minutes, and held irregular meetings and a round of social activities. Publication was sporadic; three volumes of *Mémoires* were spaced over ten years.

Crosland's material comes from libraries in several countries, perhaps chiefly British, which, as earlier work in the less formal aspects of science has shown, offer an admirable source of information and comment on French intellectual affairs. His book, deriving from both public and private archives, illuminates a period in which much that had necessarily been destroyed was being reconstructed or replaced. An important use of the book will be its accounts of the scientists of the period: Berthollet and Laplace, of course, but also Monge, Gay-Lussac, Lacépède, Arago, Thénard, Cuvier, Biot, and Chaptal, who come alive as more than merely street names on the Left Bank.

Perhaps it is inevitable that the book does not leave one with a precise picture of what the Arcueil group may have achieved. There is much on the development of chemistry, on the influence of Humboldt, on the contact of the chemists with trades and industries, on dyes, bleaches, explosives, and new elements; there is a fresh look at Napoleon and the savants, and side lights on economic and technological changes produced by the Revolution and its sequels. But the random spreading of science into many new lines of research, the complexity of personal relationships around the central figures, and the dependence of the movement on their private fortunes mean that we have here more the *View of*

French Science at the Time of Napoleon I of the subtitle rather than a monograph on a scientific organization of the more usual type.

Brown University

HARCOURT BROWN

TOCQUEVILLE AND THE PROBLEM OF DEMOCRACY. By *Marvin Zetterbaum*. (Stanford, Calif.: Stanford University Press. 1967. Pp. ix, 185. \$5.75.)

IN four connected essays, at once informative, difficult, and challenging, Zetterbaum, a political scientist, attempts to fix the theoretical structure of Tocqueville's philosophy and to demonstrate its final failure.

Zetterbaum argues that Tocqueville was more calculating and less neutral than he pretended. He never proved the inevitability of democracy or the remorseless leveling tendency of modern societies. Instead, believing that equality responded to a universal natural morality and was therefore superior to the conventional codes of aristocratic societies, Tocqueville preferred to insist that equalitarianism was "providential" and unavoidable. And so he created a "salutary myth."

Yet Tocqueville still faced the problem of reconciling natural justice with human excellence. Modern societies might be increasingly egalitarian, but they were threatened by the atomism of selfish individualism. Might not commercialism and materialism soften human egoism by the doctrine of "self interest rightly understood"? If only the individual could be taught that right conduct was also useful, and that by exercise of the American expedients of local self-government, the jury system, volunteer associations, and so forth the individual could be transformed into a good citizen, then perhaps democracy might succeed. To Tocqueville the good citizen was more important than the good man. Did he have to be religious? Religion was unquestionably useful; indeed, almost any religion was more useful than none: "Tocqueville strikes a utilitarian attitude toward religion throughout." (In this Zetterbaum differs with Doris Goldstein and myself.) Lack of space prevents recapitulation of the whole argument, but ultimately Tocqueville was not hopeful, for in his heart he realized that before democracies could cure themselves there would have to be changes in human nature.

Zetterbaum makes some interesting observations, which should spark a lively debate. But by lumping together Tocqueville's pronouncements in the two halves of his *Democracy*, and, indeed, over thirty years of a varied experience, he creates a theoretical doctrine for Tocqueville so heartless as to remind one of Machiavelli and Rousseau. Indeed Rousseau becomes the single political thinker most often mentioned: according to Zetterbaum, Tocqueville's understanding of democratic man was "based largely on Rousseau"! What we have here is not the American experiences, not Jared Sparks on town government, but *Le Contrat Social*; not a troubled and sensitive spirit but a metallic doctrine that would, I believe, have caused Tocqueville the gravest of misgivings. For he was part passion and human pity, and far from consistent.

Zetterbaum seems well acquainted with the literature, but chooses to cite

abbreviated and paperback versions of the standard studies and the English translations of Tocqueville's works rather than the originals for most purposes.

Yale University

G. W. PIERSON

LES CONSEILLERS GÉNÉRAUX EN 1870: ÉTUDE STATISTIQUE D'UN PERSONNEL POLITIQUE. By L. Girard, A. Prost, and R. Gossez. [Publications de la Faculté des Lettres et Sciences humaines de Paris—Sorbonne. Series "Recherches," Number 34. Travaux du Centre de Recherches sur l'Histoire du XIX^e Siècle, Number 1.] (Paris: Presses Universitaires de France. 1967. Pp. 211. 29 fr.)

THIS monograph concerning the personnel of French local government reveals the quantitative approach to history at its best. Using the tools of modern social science, the authors produce a statistical analysis for what had previously been nothing more than intelligent historical generalization. We see the *conseillers généraux* of 1870, save for the Seine Department, as numbers and percentages, not as living flesh. We learn who they were, their professional backgrounds, their ages, their political orientations, their lengths of service, their places of residence, and their financial bases, which the authors consider particularly helpful to know. The *conseils généraux* in the nineteenth century have received little study, perhaps because as an agency of local government their functions were limited and usually administrative. What appealed to the authors is that the number of men elected was large, but manageable (nearly 2,800), and the referendum was popular, although each *conseiller* represented an area rather than population and hence, as a whole, the *conseils* failed to be trustworthy examples of majority will. Detailed study is appropriate because complete lists for 1870 exist with ample vital statistics, plus the *liste confidentielle* indicating each *conseiller's* political persuasion as reported by his departmental prefect.

Throughout the nineteenth century the *conseils généraux* were remarkably uniform. The authors compare their figures with the results found by A.-J. Tudesq in his thesis *Les Conseillers généraux en France au temps de Guizot, 1840-1848*, and this shows little difference in background from the July Monarchy to the late Second Empire. Along with social and political stability, the *conseillers* were apolitical. The national regime could change complexion, the economic and social structure could modify, the suffrage could widen, yet, on the whole, the French local leader evidenced no change. His ideas, his income, his social background varied little as the decades passed. Once again there is ready evidence for the frustration of the urban reformer with the stability and frightening indifference of provincial political leadership.

This is a magnificent study of what can be done with the computer and other mechanical techniques in working with large numbers of men in history. We now know much that is important about the backgrounds of the *conseillers*; what is missing is how well the system worked. This was not the question asked, and for this the sources are not so easily available. When that story is written, this seminal work will be essential.

DePauw University

JOHN J. BAUGHMAN

ROGER MARTIN DU GARD: THE NOVELIST AND HISTORY. By David L. Schalk. (Ithaca, N. Y.: Cornell University Press. 1967. Pp. ix, 257. \$6.75.)

ROGER Martin du Gard is increasingly recognized as one of the greatest novelists of twentieth-century France and his two great works, *Jean Barois* and *Les Thibault*, are invaluable portraits of the periods they portray. This is especially true of *Jean Barois*, with its account of the Dreyfus years and the disillusion that followed them.

Professor Schalk's essay is difficult to situate: it is not a biography, or a life and times, or literary criticism *en règle*. Rather, it attempts to combine an account of the author and his works with an analysis of their relations to their time. Since the time that unfolds in some of the writings and the time of Martin du Gard's own life do not always coincide (as in *Jean Barois*), this complicates the problem of "the Novelist and History," the former's relation to the latter, his perception of it, his will or capacity as novelist to reconstitute and recount events deliberately chosen as the base and framework of his story; and, also, as the author makes clear, the problem of history's effect on the writer, the development in him of a "historical consciousness" highly characteristic of, and in a way peculiar to, our century.

These intriguing questions raise others in their turn, particularly that of the writer deliberately adjusting his *œuvre* to historical experience, and that of the historian attempting to use the *œuvre* for his own ends, as a kind of document. Schalk is aware of such issues, but he does not pursue them far. His book, sprinkled with acute observations, raises more questions than it answers. That could be a point in its favor if the questions were actually formulated, but, as it stands, it leaves the impression that the issue posed by the subtitle has been crowded out by an extended and ably conducted bibliobibliography. One is left wondering over the mysterious process by which historical experience, personal or vicarious, turns into evidence—or into art. How different is this transmogrification from what we historians do?

The book will join that of Denis Boak as one more valiant attempt to write about the life and work of a stubbornly discreet man who deliberately, and rightly, left his memorial in his books. Unfortunately, the English-language version of the most revealing among these books, *Jean Barois*, has been out of print far too long.

University of California, Los Angeles

EUGEN WEBER

THE ENLIGHTENMENT IN THE UNIVERSITY OF SALAMANCA. By George M. Addy. [Duke Historical Publications.] (Durham, N. C.: Duke University Press. 1966. Pp. xxi, 410. \$12.00.)

PROFESSOR Addy's book is a detailed study of the University of Salamanca from the reformed plan of studies of 1771 to that of 1807. (A lengthy appendix contains the text of both plans.) The first four chapters describe the organization and curriculum of the university since the Renaissance as a basis for appreciating the reforms. Addy's research is mainly in the university's archives, hitherto in-

sufficiently exploited, and he provides much new material on the history of Spanish higher education

Addy offers two major historical revisions: First, that the university's backwardness before 1771 was due not to inquisitorial censorship but to an entrenched system of favoritism in appointments and an antiquated distribution of chairs. Whereas historians, myself included, have singled out the introduction of new subject matter as the heart of eighteenth-century reform, Addy shows that the revision of the tenure system and redistribution of chairs were equally if not more important in modernizing the university. Second, he attacks the view that the university was reformed against its will by an enlightened royal government. Here he fails to convince me. He shows that there were enlightened professors who proposed many reforms later adopted and enforced by the crown, but at the same time his account indicates that they were in a minority at least until 1790 and that reform would have been impossible without royal authority. The university was dominated by the state in a way that would horrify the AAUP. One could logically argue that both its previous backwardness and present progress depended on the mentality of the royal government more than on the Inquisition or the faculty.

Moreover, by looking only at Salamanca, Addy gives the impression of more initiative within the faculty than is warranted. Enlightened Spaniards were far ahead of most Salamanca professors, and other universities, notably Valencia and Seville, achieved more profound reforms. The Salamanca faculty simply had to change to maintain their pre-eminence in Spain.

My major complaint, however, is that the book does not discriminate between the important and the trivial. By trying to be all-inclusive, Addy forces the reader to wade through pages of inconsequential detail in search of major developments. But for this the editors must share the blame with the author.

University of California, Berkeley

RICHARD HERR

THE CARLIST WARS IN SPAIN. By *Edgar Holt*. (Chester Springs, Pa.: Dufour Editions. 1967. Pp. 303. \$7.50.)

EDGAR Holt's book covers the Carlist Wars from 1833 to 1876, but the major portion concerns the first war in the 1830's. A final chapter describes the evolution of Carlism and the line of succession in the twentieth century. The book is intended for the general public more than for professional historians; it has no footnotes, and, except for a few Spanish titles, the works cited in the bibliography are all in English. Holt does, however, make good use of the British diplomatic dispatches from Spain in the 1830's.

The central subject of the book is the military side of the wars, including the vicissitudes of the mercenary British Legion recruited in England in 1835 by partisans of Isabel II. Much space is given also to the personalities of the Carlist pretenders, of their leading generals, and also of Queen Isabel, whom they opposed but whose licentious and unfortunate life fascinates the author. Running through the story is also a surprisingly good history of Spain at the ministerial level. What is missing, however, is any attempt to penetrate the social, economic,

and ideological factors that brought on the wars or that accounted for the Carlist failure. Holt recognizes Carlism as a rural phenomenon that favored traditional values and opposed the industrialism and anticlericalism of the cities, but there is nothing original or very profound in this observation.

On the other hand, the book makes very pleasant reading, and I learned much about the events of the wars and their leading figures. It can be useful for collateral reading in modern Spanish history.

University of California, Berkeley

RICHARD HERR

GERMAN ECONOMIC POLICY IN SPAIN DURING THE SPANISH CIVIL WAR, 1936-1939. By *Glenn T. Harper*. [Studies in European History, Number 13.] (The Hague: Mouton & Co. 1967. Pp. 150. 21 gls.)

PROFESSOR Harper's doctoral dissertation is the most extensive study that has been made of German economic policy vis-à-vis Nationalist Spain during the Civil War. It is commendably objective, free from the loaded adjectives that even professional scholars sometimes cannot resist when writing about the Spanish Civil War. With one or two exceptions, its details seem accurate, and its ultimate conclusion—that Franco escaped diplomatic and economic subjection to Hitler—is indubitable. Yet this is so well known that it raises some question concerning the importance of Harper's theme, for the record he narrates would appear to indicate that political interest and vague ideological sympathies, rather than determination to exact economic concessions, were the main motivations for German assistance to the Nationalists. The author does not fully resolve this point, possibly because it might detract from the emphasis of his book. His main achievement, though not his primary concern, is to place German assistance in slightly better perspective with regard to the general progress of the war than has been done in broad accounts of the conflict. Except for a few details and insights related to this, the story he tells will be perfectly familiar to those who have read the other books on the Spanish Civil War published during the past decade. In fact, there is not too much here that could not be gained from a reading of the volume of German documents on the Civil War published in 1950, to which nearly half the footnotes in this book refer.

The research was apparently restricted to published materials, and there is no indication that unpublished German documents were consulted. There are also lacunae in the use of published works. The author apparently did not read Helmut Dahms's history of the Civil War, which, though somewhat tendentious, made greater use of German sources than did other general accounts; nor does he seem to have consulted Marion Einhorn's Marxist study of the economic background of German intervention or Robert Whealey's dissertation on Germany's diplomatic and economic relations with Spain in 1939, which provides a more thorough study of the final phase of German economic policy during the Civil War.

University of California, Los Angeles

STANLEY G. PAYNE

HISTOIRES DE BELGIQUE. By *Carlo Bronne*. (Paris: Librairie Plon. 1967. Pp. 322.)

UNLIKE Pirenne's *Histoire de Belgique*, but similar to Rudel's *Histoires de Bretagne*, Carlo Bronne's sixty-seven essays use the kaleidoscopic method of recounting the past. The author, a distinguished member of the Belgian Royal Academy of the French Language and Literature, is not a trained historian, yet he has produced several interesting works on the modern Belgian state and its monarchs. This new cultural history is difficult to reject, for, although much of it is anecdotal, episodic, and even superficial, it nevertheless has important enlightening moments. The variety of ground covered makes the book hard to summarize concisely, but generally enlarges its appeal.

No one could be a specialist in this area, and no one can command all the varying kinds of evidence specialists would bring to bear on it. But Bronne is a savant, a historian of a people, of their ideas, and of their evolving culture. Fortunately he travels an unexpected path over some old, and many new, fences of the broad Belgian historical time line. His work offers no analysis or overriding theme; Bronne is particularly helpful, however, in providing both insights and illustrations to the long, rich, diversified, and vexed history of the Flemings and the Walloons. The short essays, which average five pages, invite attention in the fields where Bronne is most skillful, such as royalty portraits (Hortense, Elisabeth, the two Leopolds, and Albert) and literary figures (Hugo, Verlaine, Verhaeren). The entire collection is primarily to be appreciated in the sections on the Spanish rule and the revolutionary-Napoleonic eras. Many English-language historians could learn from Bronne's majestic and incisive prose in his description of the brutal murder of the Liège *bourgmestre* in 1637 or the beauty-horror of the Waterloo Ball in 1815.

Two strong complaints should be entered, neither a niggling criticism. The author has unquestionably sacrificed depth for breadth, leaving the reader with a frustrated wish for deeper penetration into his subject matter. A more balanced and valuable work might have emerged with severe cutting of the lightweight essays and detailed amplification or expansion of the others. Furthermore, the entire volume is nude of notes or references, although there is a slim bibliography. Bronne's no-nonsense-about-scholarly-documentation attitude is a deplorable omission for the student who hopes to probe further.

Tulane University

PIERRE HENRI LAURENT

DIE STELLUNG DES ERASMUS VON ROTTERDAM ZUR RÖMISCHEN KIRCHE. By *Georg Gebhardt*. (Marburg an der Lahn: Oekumenischer Verlag Dr. R. F. Edel. 1966. Pp. 440.)

IN this dissertation Dr. Gebhardt seeks to further our understanding of Erasmus' personality and devotion by an investigation of his attitude toward the Roman Church. Previous research, according to Gebhardt, has concentrated too heavily upon Erasmus' criticism of the Church and has neglected the positive elements in his theology of the Church.

Gebhardt divides the writings of Erasmus into three periods, the striving for

a deepened Christianity through ancient thought, reaching a high point about 1501; the laying of the foundations of his later theological productivity, up to about 1514-1515; and the creation of a comprehensive theology, 1514-1535. Gebhardt finds little of Erasmus' ecclesiology in the first two periods; consequently, he draws mainly upon writings from the third period, as well as upon numerous scholarly works by modern authors

Erasmus is presented here as an orthodox Catholic whose view of the Church conforms to traditional doctrine. Gebhardt holds that Erasmus was not an opportunist yielding to his Catholic patrons; nor was he a mere undogmatic moralizer for whom the Church has no significant role. Erasmus wanted reform, but he differed from Luther in aiming at changing not the institution but its members. Gebhardt contrasts Luther and Erasmus in a number of ways: the "religious man" with the scholar, Luther's insistence upon faith alone with Erasmus' trust in faith and reason, Luther's reliance upon Scripture alone with Erasmus' search for God's revelation both in the Bible and in tradition. But Erasmus' devotion was no less than that of Luther, and both Erasmus' early criticism of the Roman Church and his later loyalty to it rested upon deep theological conviction.

While Erasmus conceded that the universal and invisible Church includes some who have never heard the Gospel, baptized Christians must remain true to the Church, the proper form of which is the old hierarchical institution. Gebhardt believes Erasmus was a moderate papalist, attributing the highest teaching function to councils (consistent with his emphasis upon consensus in matters of tradition) and the highest pastoral authority to the pope.

Reformation historians will find much of interest in Gebhardt's dissertation, for it presents Erasmus' relationship to the Reformation from a different perspective and points to the nature of the Church as an important issue.

Berea College

FRANK J. WRAY

THE DUTCH SEABORNE EMPIRE: 1600-1800. By C. R. Boxer. [The History of Human Society.] (New York: Alfred A. Knopf. 1965. Pp. xxvi, 326. \$6.95.)

PROFESSOR Charles R. Boxer, in his book, *The Dutch Seaborne Empire: 1600-1800*, presents the story of the rise of the Netherlands' provinces of Holland and Zeeland to the position of the principal seafaring area of the world in the seventeenth century. He explores the causes of this success and makes a convincing case for the interaction of the ruling classes in the Netherlands with the lower classes in achieving the domination of European seaborne commerce as well as the expansion of the Dutch overseas.

The great achievements of the Dutch East India Company (VOC) are described, and the absorbing interest of the *Heeren XVII* in trade and profits to the exclusion of empire and conversion of the natives to Christianity is well defended as being one of the main reasons for Dutch success in the East Indies. The differences in purposes between Dutch activity in the East and that of the English and Portuguese is demonstrated. The Dutch West India Company receives fuller treatment than it deserves in comparison with the VOC.

One of the fine features of the book is the skill with which Boxer balances and contrasts the overseas activities of the companies with the ambitions and interests of the ruling classes and merchants at home. The work deals not only with the overseas Empire and the life and work of the Dutch, particularly in the Indies; one also gains a feeling of the ruling classes of the Netherlands and their attitude toward profits and trade. The brief explanation of the government of the Dutch Republic in the seventeenth century is particularly good and necessary to demonstrate the position of the merchant class in both government and commerce.

The last chapter deals with the causes of the rapid decline of the Dutch East India Company in the eighteenth century until its final dissolution in 1795. The exhaustion of the small nation of Holland in its long series of wars with England beginning in 1652 and continuing until 1784 is not the real cause of the decline of the VOC. The stagnation and decrease of the population, the decline of the coastal fisheries, the lack of sufficient men trained to life at sea, all contributed to make the eighteenth century a shadow of the glory of the seventeenth century.

A fine chronology is included, but the index leaves much to be desired. The bibliography contains the most important titles in both English and Dutch concerning Dutch seaborne activities covered by the period. A question can well be raised as to why Dutch activity in Japan, where the Dutch held such a unique position from 1641 to 1854, is so briefly touched upon. It would make for better balance if this activity were given more attention.

This book is recommended to anyone who seeks a brief, clear account of the "Golden Century" of Dutch sea power, the causes for its rise and for its decline.

Allegheny College

PAUL B. CARES

STANDEN EN LANDEN: WETENSCHAPPELIJKE BIJDRAGEN UITGEGEVEN DOOR DE BELGISCHE AFDELING VAN DE INTERNATIONALE COMMISSIE VOOR DE GESCHIEDENIS VAN STANDEN EN LANDEN (NATIONAAL CENTRUM VOOR NAVORSING, V.Z.W.) ONDER DE HOGE BESCHERMING VAN DE PROVINCIALE OVERHEID VAN ANTWERPEN, BRABANT, LIMBURG, OOST- EN WESTVLAANDEREN. Volume XLI. (Louvain: Uitgeverij Nauwelaerts. 1966. Pp. 265, 41. 480 fr. B.)

THE Belgium that emerged from the Revolution of 1830 as an independent nation for the first time in its history did not lack experience in representative government, a type of government it had known at least since the Burgundian period in the fifteenth century, but it did lack experience in making parliamentary government succeed with a king of its own choice who, by the Constitution of 1831, was specifically made a constitutional monarch. Although under the Burgundians, the Habsburgs, Napoleon, and William of the Netherlands the Belgians had demonstrated their political capacity and faith in self-government, the question after 1831 was whether they could transform the success of 1830-1831 into permanent success. Essentially this is the subject of the present six essays contributed by members of the Belgian section of *La Commission Internationale pour l'Histoire des Assemblées d'États*.

Except for the first essay by Émile Lousse concentrating on a definition of representation and on the development of representative assemblies from the medieval estates to the modern parliament, the other essays discuss the Belgian system of government as it evolved between 1831 and 1964. The principal value of Lousse's essay is its treatment of the legal, political, and economic disabilities that have historically prevented people from voting or from participating in representative government, and that would be entirely removed only in the twentieth century. At the end of the first century of Belgian independence parliamentary government was firmly entrenched, and, Lousse suggests, Brussels might well emerge as the permanent capital of supranational assemblies which will ultimately replace national parliaments.

In his essay on the Belgian Parliament as representative of the whole nation, Lode Wils traces the steps by which the ingrained Belgian regionalism and particularism yielded to the feeling that the nation rather than regions should be represented. Related to this essay is that by P. Maurice Orban on the parliamentary system of Belgium, which traces the attempts of social, economic, and political groups to control Parliament. He emphasizes that in actual Belgian parliamentary practice few members represented the whole nation or the common good. Perhaps Edmund Burke was right when he observed that "common interest and common sentiment are rarely mistaken," but he was wrong when he stated that they, rather than constituents, were represented.

Fernand Lehouck examines the role of the crown in obtaining parliamentary support for military defenses along the eastern frontier and for the establishment of an army to provide minimum security for a nation whose territorial integrity seemed less secure in the period after 1870. The chief obstacles to the build-up of the military were complacency and neutralism stemming from reliance on the guarantees made by the Great Powers in 1839. Since in Parliament the most consistent opponents of the crown were liberals and socialists, it was from the conservative elements that Leopold II secured his army and his forts along the Meuse. It was, however, by astute politics, not by autocratic pressures, that Leopold finally achieved his limited objectives. Parliamentary principles were never violated.

Most stimulating is the essay by Theo Luykx on the attempt of Napoleon III to influence the election for the Belgian chamber on June 8, 1852. Angered by the asylum afforded by Belgium to such as Thiers, Victor Hugo, and Proudhon, by the freedom of expression given to his opponents, and by the articles appearing in such papers as the *Étoile Belge*, Napoleon III threatened Belgium with the end of economic relations and worked to secure the election of Belgian senators sympathetic to him. When the conservative candidates with whom he cooperated failed to secure a majority, Napoleon ended his foray into Belgian politics.

These essays underscore the similarity in the methodology of the historian of modern institutions and the political scientist. Both ask basically the same questions; both attempt to find the answers by systematic analysis of historical evidence.

Brown University

BRYCE LYON

BESCHEIDEN BETREFFENDE DE BUITENLANDSE POLITIEK VAN NEDERLAND, 1848-1919. Third Period, 1899-1919. Part 5, 1917-1919. Volumes I and II. Edited by C. Smit. [Rijks geschiedkundige Publicatiën, Major Series, Numbers 116 and 117.] (The Hague: Martinus Nijhoff. 1964. Pp. xvi, 642; 646-1122.)

THE fifth and last part of this series pertaining to Dutch foreign affairs between 1848 and 1919 includes documents dealing with events between January 31, 1917, the beginning of unrestricted U-boat warfare, and June 28, 1919, when the Versailles Treaty was signed. The documents are mainly drawn from the Ministry of Foreign Affairs, but some also come from the private archives of leading Dutch personalities, the correspondence of the Dutch high command, and communications between Dutch authorities concerned with the administration of the East Indies.

The nearly eleven hundred items in the last two volumes of this series cover a great variety of diplomatic activities and may prove very useful to experts in search of highly selective information. They will, however, hardly hold the interest of the reader who might have hoped for a fascinating "inside story" of diplomatic action converging on and emanating from one of the few neutral European capitals and one of special strategic sensitivity. Although there is a wealth of documents in four languages and coming from the four corners of the world, much of the material deals with the day-to-day routine diplomatic correspondence of a small country in a precarious position of neutrality engulfed by a warring world.

A topical arrangement of the materials might have helped to overcome tediousness and would certainly have enabled the reader to get a clearer over-all view of the more significant developments. A good subject index at the end of the last volume serves indeed as a belated guide; it is matched by an excellent index of persons. There is also, in the introduction, a useful list of Dutch diplomats serving abroad during the period under review.

In order to obtain a meaningful overview of the most important events I found it useful to group the materials under certain headings. Neutrality, quite naturally, forms the largest part of such topical arrangement. There is a heavy concentration on diplomatic exchanges concerning Dutch neutrality in connection with the imposition of unrestricted submarine warfare. British assurances of continued trade mingle with Dutch protests over British mine laying near the Dutch coast and German apologies for the sinking of Dutch merchantmen. The neutrality issue once more assumed formidable proportions with the flight of the Kaiser and the withdrawal of German troops through Limburg Province.

Another major topic is that of peace negotiations. Earliest documentation in this collection dates to mid-September 1917, in an exchange between Lord Bryce and the Dutch Foreign Minister during which the former regretted German "stubbornness." In an interesting document of August 19, 1918, Mr. Churchill was reported to have insisted that the Germans be beaten decisively and voluntarily forego their system of government. The Dutch ambassador to Berlin wrote at the end of October that "the great creation of Bismarck was sinking," and on October 30 he first mentioned the possibility of the Kaiser's abdication.

The League of Nations formed yet another subject of considerable importance in the last part of the collection. As early as January 21, 1919, the Dutch ambassador to Paris reported the US Senate's opposition to incorporation of the League Covenant in the treaty and President Wilson's determination to force the issue. Noteworthy, too, is the growing interest of the Dutch government to enter the League, even though there was some uneasiness regarding French dominance.

Dutch-Belgian tensions in 1919 over Belgian claims to the Schelde estuary and parts of Limburg are another issue of considerable diplomatic importance. The Belgian demand for a revision of the Treaty of 1839 greatly irritated the Dutch. The British were reported to back the Dutch while the French at least temporarily sided with the Belgians. As late as April 1919 King Albert I was reported to be unfavorably disposed toward the Dutch because they had not protested the German invasion in 1914 and had permitted German troops to withdraw through Limburg in 1918. Eventually the controversy was shelved due to moderating counsels mainly among the Big Four at Versailles.

American University

F. GUNTHER EYCK

COMMUNISM IN FINLAND: A HISTORY AND INTERPRETATION.

By *John H. Hodgson*. (Princeton, N. J.: Princeton University Press. 1967. Pp. ix, 261. \$6.00.)

THE author surveys his subject in eight chapters. About one-fourth of the book is devoted, surprisingly, to the political labor movement before January 1918. Holding that the strength of Finnish Communism, "past as well as present, is in large measure a reflection of the nature of Finnish Social Democracy," he attempts to explain the role of the Communists in the elections of 1945-1966 and offers explanations for the prominence of the Communist-dominated SKDL since the end of the last war (23.5 per cent of the popular vote in the 1945 election, which the author calls a "resounding victory," and 21.2 per cent in that of 1966).

Professor Hodgson's heavily footnoted and detailed survey and explanations fail, it seems to me, to take note of a number of factors and circumstances that must be considered in any attempt to answer the questions he raises. One of them would appear to be the decision of the Finnish Communists, formulated over two decades ago, to campaign and labor for their cause not as the Communist party but behind the façade of the Finnish People's Democratic League (the SKDL). Another is the obvious fact that in Finland anti-Communist discussion, debate, and activity in general have been restricted especially by the desire not to arouse Soviet suspicions or enmity. This has given the Communists a position and advantages that pre-1945 circumstances would not have given the party. A third has probably been the persistence, in some parts of the country, of the divisive effects of the legacy of the Civil War in 1918 which, incidentally, the author sees primarily as a conflict between "The Reds and The Whites," and not as a civil war launched against the legal government by radicals who had managed to capture the leadership of the Social Democrats and of the labor union movement. Finally, the attempt to measure the importance of Communism in Finland requires recognition of an interesting fact: Finnish intellectuals as a group have

rejected the Communist version of the gospel according to St. Marx. This has meant, among other things, that the Communist cause in general and the SKDL in particular have lacked broad-fronted, intellectually impressive, and persuasive leadership. It may also account for the acceptance by the Communists since the last war of the ways of parliamentary democracy instead of having recourse to militant "direct action." Thus it would seem that the "nature of Finnish Social Democracy" furnishes at best only a partial explanation of post-1944 Communism in Finland. It also leaves open the interesting question of why the numerically small Communist party—estimated at about 40,000–45,000 members—has since 1945 been able to capture, by means of the SKDL, approximately ten times that many voters in national elections.

Columbia University

JOHN H. WUORINEN

DANMARKS UDENRIGSPOLITIK, 1933–1940. By *Viggo Sjøqvist*. [Udgivelseselskab for Danmarks nyeste Historie.] ([Copenhagen:] Gyldendal. 1966. Pp. 417.)

A DANISH society, enjoying firm governmental and foundation backing (*Carlsbergfondet*) and organized to support research into "Denmark's newest history," in the sixties has been sponsoring several studies on the occupation period (1940–1945). The present volume, on the antecedent years, is part of this venture. Its author, best known previously for a two-volume biography of a key nineteenth-century official in the Foreign Office, P. Vedel, is now chief archivist in the Danish Ministry of Foreign Affairs.

Foreign policy planning in Denmark is more than normally complicated by geography. In effect, the country is an archipelago. The land frontier is only 42 miles long while the shore lines, including those of 99 inhabited and over 380 uninhabited islands, run to over 4,600 miles. Neutrality is not easily guarded here. Moreover, the country sits athwart the entrances to the Baltic, one of the major shipping seas, and next door to a Great Power neighbor. Planning for defense, neutrality, or both, can make foreign policy an involved matter.

This study shows how diversified, in a democratic society, are the elements that play into the formulation of foreign policy. On issue after issue we are led patiently through discussions in many quarters: in parliamentary sessions and committees, in political party councils, even to some extent, into press debates, foreign at times as well as domestic. For the years in question, there was an added sense of growing danger, as the dictators slashed away at the weakening fabric of international order. To the policy makers at Copenhagen it was evident that if war came there would be no assistance for their country, neither from Geneva, from England, nor even from their close Scandinavian neighbors. In this context, one may place the disputed Nonaggression Pact of 1939 with Germany.

Two individuals figure prominently in this study: Premier Stauning and Foreign Minister Munch. The latter emerges as a very cautious figure, often apprehensive lest Norwegian or Swedish colleagues push too actively for a joint *Norden* policy, and uniformly reluctant to have the League at Geneva involved when a disputant was one of the Great Powers; this was a point of some im-

portance during Denmark's three-year term (1933-1936) on the Council of the League.

Though his subject at times grows complicated, Sjøqvist keeps his narrative consistently clear. His coverage of sources is wide, especially in unpublished Danish and German archival materials. This study deserves a good index, the present one being little more than a glossary, identifying the figures treated in the text. In one area the textual treatment also is sparse; it says little of discussions the Minister must often have had on critical issues with important personnel in his own ministry. The tone of the discourse is calm and objective, not least so when discussing Berlin's reactions toward local Nazis in Slesvig. Only in a few concluding paragraphs does the author betray uneasiness over some matters of larger policy. Rather modestly he calls his study a "sketch" and laments the continuing inaccessibility of various materials. No doubt in time we will have learned more about individual matters, but the larger pattern of the considerations that entered into the formulation of Denmark's foreign policy during Hitler's provocative years seems firmly established here.

New York University

OSCAR J. FALNES

SVENSK UTRIKESPOLITIK EFTER 1945. By *Nils Andrén* and *Åke Landqvist*. (Stockholm: Almqvist & Wiksell, 1965. Pp. 294. S. Kr. 19.50.)

SWEDEN's painstaking formulation of its policy of "freedom from alliances in peace implying neutrality in war" gives this volume its importance for the diplomatic historian. Although its purpose is avowedly pedagogical, it contains, in Swedish, what the foreign student and teacher need: an expertly written summary of Swedish diplomatic history since 1500, followed by forty-seven well-chosen documents illustrating the period since 1945. These committee reports, statements by Foreign Ministers, Prime Ministers, ambassadors, and others indicate not only the methods by which the public has been informed and influenced, but the processes of policy formation through which the Swedes have attained practical unanimity among the parties on foreign policy. The treatment would have been strengthened by inclusion of the press debate, but at the expense of considerable bulk. Obviously business and public opinion affects government action relative to matters like EEC and EFTA and Nordic cooperation, and this is only partly reflected in official statements that make answer.

Especially interesting are the documents that indicate policy modifications in connection with the United Nations and EEC, and the rationale of Sweden's eagerness for a Nordic defense organization but its rejection of alignment with any Great Power grouping. The volume is really an elaboration of a government statement of 1949: "When we attempt to solve our security problem in our own way, we are naturally aware that there is no solution which in the present situation provides complete satisfaction." The value of a collection such as this is the insight it gives into the difference in outlook between the small nation, conscious of its achievements but also of its impotence in the international arena, and the great states perhaps overly conscious of both their power and their responsibilities.

Northwestern University

FRANKLIN D. SCOTT

DER DEUTSCHE ORDEN AM MITTELMEER. By *Kurt Forstreuter*. [Quellen und Studien zur Geschichte des Deutschen Ordens, Number 2.] (Bonn: Verlag Wissenschaftliches Archiv. 1967. Pp. 283. Cloth DM 29.80, paper DM 26.80.)

DER DEUTSCHE ORDEN IM ZEITALTER NAPOLEONS. By *Friedrich Täubl*. [Quellen und Studien zur Geschichte des Deutschen Ordens, Number 4.] (Bonn: Verlag Wissenschaftliches Archiv. 1966. Pp. 204. Cloth DM 23.80, paper DM 20.80.)

THESE two volumes are parts of a new series devoted to the publication of source material and historical studies related to the history of the Order of Teutonic Knights (*der Deutsche Orden*).

The volume by Dr. Forstreuter, former director of the Prussian State Archives at Königsberg, is concerned with the activities of the Knights in the Mediterranean area and the Near East. While one ordinarily thinks of the activity of the Teutonic Knights as carried on principally in Prussia and the Baltic area in connection with the German push to the east in the thirteenth to the fifteenth century, it is noted that the order, founded at Acre in 1190, continued its activities in Palestine until the fall of Acre in 1291, and still later in the other crusading states, Cyprus, Greece, Italy, Sicily, Provence, and Spain. The headquarters were at Acre until 1291 and thereafter at Venice until 1309. Only after 1309 were they transferred to Marienburg. The widely scattered holdings in the Mediterranean area continued to be held under the direct jurisdiction of the Grand Master of the order.

Many properties, especially in Apulia and Sicily, were originally received from the Hohenstaufen kings and emperors. The principal house of the order in Sicily was the church known as La Magione in Palermo. In Apulia the order's headquarters were first at Barletta and then at San Leonardo at Siponto. By the close of the Middle Ages most of the order's scattered properties in the Mediterranean area had been lost, except at Rome, where a representation continued until 1809.

Forstreuter's volume is largely based on documentary sources, chiefly those of the Central Archives of the order in Vienna and the State Archives formerly in Königsberg.

Dr. Täubl's volume is also based on extensive archival material from the Central Archives of the order in Vienna and from the *Haus-, Hof-, und Staatsarchiv* there. It deals with the difficulties of the order during the Napoleonic Wars, the loss of most of the order's German possessions to Bavaria, Württemberg, and Baden, and the dissolution of the order in all the states of the Confederation of the Rhine by Napoleon in 1809. By the terms of the Treaty of Pressburg of 1805, the Grand Master was to be an Austrian archduke designated by the emperor. The order's Austrian possessions continued to be held by it, the Austrian emperor thereafter acted as its protector, and the seat of the Grand Master was in Vienna, where it has remained to the present day.

The two volumes reviewed here indicate that the series, under the editorship of Dr. Klemens Wieser, is being produced under high scholarly standards.

Fort Lewis College

JAMES S. BEDDIE

BERLINER GROSSKAUFLEUTE UND KAPITALISTEN. Volume I, BIS ZUM ENDES DES DREISSIGJÄHRIGEN KRIEGES, by *Hugo Rachel et al.*; Volume II, DIE ZEIT DES MERKANTILISMUS, 1648–1806; Volume III, ÜBERGANGSZEIT ZUM HOCHKAPITALISMUS, 1806–1856, by *Hugo Rachel* and *Paul Wallich*. Revised by *Johannes Schultze et al.* [Veröffentlichungen des Vereins für Geschichte der Mark Brandenburg.] (Berlin: Walter de Gruyter & Co. 1967. Pp. xli, 415; vii, 578; 336. DM 58; DM 64; DM 34.)

THIS publication is, except for an account of its genesis and an up-to-date bibliography, essentially a reprint of the original work that appeared in Berlin in 1934–1939. It is in a sense dedicated to the memory of Paul Wallich, who was one of its main contributors and its sponsor and who ended his life under tragic circumstances in 1938. But one would fail to honor his memory if one were not to judge his work on its own merits.

This reprint is particularly welcome since only few copies of the original Volumes II and III have survived Nazi persecution and since the extensive archival material, which forms the foundation of this work, is no longer available in its entirety.

Providing a cross section through the economic life of Berlin in its formative period, the work gives a detailed and careful account of the development of big business there. While it contains a wealth of information on the various firms and while it also interprets and analyzes these data with sound judgment, it does not extend its evaluation to questions pertaining to the etiology of economic growth. Yet, it furnishes so much economic, sociological, and genealogical information that it could be used effectively in the study of the interrelationships of these various factors. The work also makes excellent supplementary reading for those surveying general Prussian history. I found Volume II, which covers a period of close interaction between government and big business, particularly interesting. In conclusion, one can only say that no man could ask for a finer memorial than this edition.

Library of Congress

ARNOLD H. PRICE

REGIERUNG UND VERWALTUNG: GESAMMELTE ABHANDLUNGEN ZUR STAATS-, RECHTS- UND SOZIALGESCHICHTE PREUSSENS. By *Otto Hintze*. Edited and explained by *Gerhard Oestreich*. [Gesammelte Abhandlungen, Volume III.] (2d rev. ed.; Göttingen: Vandenhoeck & Ruprecht. 1967. Pp. 31, 675. DM 48.)

Otto Hintze (1861–1940) served for many years as editor of the *Acta Borussica* and of the *Forschungen zur Brandenburgisch-Preussischen Geschichte*. His research in documents concentrated upon Prussian history, but his title, professor of constitutional, administrative, economic history and politics, at the University of Berlin indicates the range of his interests. In a succinct and informative introduction Professor Gerhard Oestreich describes Hintze's extensive studies as a young scholar in comparative history and states that Hintze wished to be known

not merely as a historian of Prussia but as a scholar in the broader field, European and extra-European.

The range and depth of his interests and the originality of his insights in the difficult and exacting field of his researches entitle Hintze to a place among the most distinguished historians of this century. Oestreich points out that Hintze kept his studies closely related to political history and that he did not lose sight of the significance of personalities in shaping history or of the ultimate objective of portraying the total culture. Hintze originally had ambitious plans for a large work of synthesis. This project he never carried out, perhaps because the political fate of Germany and his own failing eyesight discouraged him. With one exception his best work consisted of articles, the majority of which have been republished in three volumes; this one is the last.

The volume offers with one omission all the studies contained in the first edition published in Leipzig in 1943 under the title *Geist und Epochen der Preussischen Geschichte* and long since unavailable. The first article appeared in 1896, the last in 1931, and all are essential. They should be read in conjunction with the articles in the other two volumes, and I suggest that anyone unacquainted with Hintze's ideas begin with the studies in Volume I on general constitutional history.

All of Hintze's *œuvres*, including his one-volume *Die Hohenzollern und ihr Werk*, deserve to be republished. I urge that the articles omitted from these three volumes and the monumental book-length study of *Bureaucratic Organization and General Administration in Prussia at the Accession of Frederick the Great*, a volume that appeared in the *Acta Borussica* in 1901, be reprinted. Hintze deserves an edition of all his work, including his correspondence.

University of California, Los Angeles

EUGENE N. ANDERSON

ZWISCHEN REFORM UND REVOLUTION: LAGE UND KÄMPFE DER SCHLESISCHEN BAUERN UND LANDARBEITER IM VORMÄRZ 1840-1847. By *Helmut Bleiber*. [Deutsche Akademie der Wissenschaften zu Berlin, Schriften des Instituts für Geschichte. Second Series, Landesgeschichte, Number 9.] (Berlin: Akademie-Verlag. 1966. Pp. 245. DM 28.)

BLEIBER's work is the fruit of an extensive search of records, contemporary publications, and historical treatises. His aim is to fill part of the gap left by the "insufficient treatment" of this subject by "bourgeois historiography." He does this by fighting this alleged bias with an unconcealed bias of his own. He relies not only on uncorroborated statements by partisan observers, but also on a fragmentary, if not erroneous, factual framework, especially in regard to statistics. The result is a one-sided and inconsistent interpretation that does not convince. One misses, in particular, a detailed discussion of the economic forces at play and their impact on agricultural production in Silesia. Specialists who cannot easily avail themselves of the sources will probably be able to distill useful information from this work, but others will get little from the ever so fluid description of "die Creme der blaublütigen Gesellschaft, die das Landvolk aussog."

Library of Congress

ARNOLD H. PRICE

AGRARISCHE INTERESSENPOLITIK UND PREUSSISCHER KONSERVATISMUS IM WILHELMINISCHEN REICH (1893-1914): EIN BEITRAG ZUR ANALYSE DES NATIONALISMUS IN DEUTSCHLAND AM BEISPIEL DES BUNDES DER LANDWIRTE UND DER DEUTSCH-KONSERVATIVEN PARTEI. By *Hans-Jürgen Puhle*. [Schriftenreihe des Forschungsinstituts der Friedrich-Ebert-Stiftung. Series B. Historisch-politische Schriften.] (Hanover: Verlag für Literatur und Zeitgeschehen. 1966. Pp. 365.)

THIS work is a scholarly study of the *Bund der Landwirte*, an important organization of German agriculturists founded in 1893 to protect farming interests against hard times and the liberal trade policies of Chancellor Caprivi. The author describes in detail the morphology and ideology of what he considers the first modern pressure group in the politics of Central Europe. Its flatulent rhetoric about national greatness and social justice served to disguise the designs of an aristocratic caste seeking to preserve its privileged position. While claiming to speak for all farmers, it actually represented the great landowners east of the Elbe. While pretending to be above party politics, it remained closely allied with the Conservatives. While posing as the champion of tradition, it fought for the maintenance of class rule. Everything it did reflected the desperate struggle for self-preservation of a hierarchical social system threatened by the rise of industrialism.

The *Bund der Landwirte* was one of many conservative civic and economic organizations that raised slogans of corporatism and chauvinism to oppose the democratization of public life. It sought the support of the peasantry and the petty bourgeoisie by promising to save them from the clutches of finance capitalism, by which they felt threatened. It idealized the virtues of rural life and small-scale enterprise; it piously invoked throne and altar; it thundered against democracy, plutocracy, and socialism; it indulged in racialism and anti-Semitism. But it could not, before 1914, halt the movement of German politics to the Left. After the fall of the Empire, however, its precepts found willing converts during the bleak years of the republic.

The author's analysis of the *Bund der Landwirte* is on the whole very persuasive, although he tends to overstate his case. To make the organization primarily responsible for the transformation of the Conservative party from a patrician defender of the legitimist ideology into an opportunistic panderer to popular prejudice is going too far. All parties were forced at the turn of the century to adapt their tactics to the emergence of a mass electorate. The doctrines of plebiscitary authoritarianism increasingly favored by the Right represented an attempt to defend class interests that appeals to morality and history could no longer protect. The change in the political structure and ideological content of conservatism was not basically due to the agitation of agrarian demagogues, but to the altered conditions of civic and economic life.

University of Wisconsin

THEODORE S. HAMEROW

LUDENDORFF UND DIE MONOPOLE: DEUTSCHE KRIEGSPOLITIK 1916-1918. By *Hellmuth Weber*. [Deutsche Akademie der Wissenschaften zu Berlin, Schriften des Instituts für Geschichte. Series 1, Allgemeine und deutsche Geschichte, Number 28.] (Berlin: Akademie-Verlag. 1966. Pp. 174. DM 12.)

This copiously annotated monograph presents some new factual information, chiefly from government files and *Nachlässe* in East and West German archives, and a rather simplistic Marxist-Leninist analysis of German politics during the First World War. The book appeared at the same time as Gerald D. Feldman's *Army, Industry, and Labor in Germany, 1914-1918* (1966), which more convincingly covers much of the same ground, though Feldman, unfortunately, had only limited access to the East German archival material.

Weber starts out with the observation that World War I significantly accelerated the transformation of "monopoly" into "state monopoly" capitalism, the latter being generally characterized by an "intimate blending" of the state with capitalist organizations, public regulation of production and distribution, and compulsory employment practices. In Germany, he contends, these wartime developments were very advantageous to the "monopolies" (big industrial concerns, in particular), for with the direct assistance of the government and the acquiescence of certain misguided labor leaders they could intensify the exploitation and "enslavement" of the working masses. This nefarious system took shape during the first two years of the war and reached its culmination with the appointment of Hindenburg and Ludendorff to the high command and the resultant adoption of "total war" programs. According to Weber, the 1916 change in the OHL was arranged mainly by Germany's leading capitalists, who counted on Ludendorff and his staff, especially Colonel Max Bauer, not only to secure military victory and imperialist prizes abroad, but also to protect the interests of the "monopolies" at home: that is, to assure them of maximal profits and to keep the working class in its place. During the remainder of the war, Weber concludes, the OHL, and other government agencies as well, did indeed follow closely the wishes and demands of the "monopolists"; disagreements between them revolved mainly around questions of "tactics."

It is well known that many German industrialists wanted an annexationist "victory peace" and favored a reactionary domestic policy; it is also undeniable that some of them made huge profits during the war. Yet, Weber's contention that Germany's wartime policies were shaped primarily by the "monopolists" and that the third OHL was their "executive organ" is based on a rather arbitrary interpretation of his own evidence. Given the impressive range of his research, it is regrettable that Weber's approach should have been so doctrinaire.

University of Alberta

ULRICH TRUMPENER

DAS PROBLEM DER KONTINUITÄT IN DER DEUTSCHEN REVOLUTION: DIE POLITIK DER STAATSEKRETÄRE UND DER MILITÄRISCHEN FÜHRUNG VOM NOVEMBER 1918 BIS FEBRUAR 1919. By *Wolfgang Elben*. [Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, Number 31.] (Düsseldorf: Droste Verlag, 1965. Pp. 194. DM 32.)

THIS study, treating the policies of the state secretaries of the *Reichsämtler* and of the military leadership between the crucial months of November 1918 and February 1919, is a significant addition to the historical literature on the German Revolution. It does not exhaust the subject, but it reveals new information and supplements earlier accounts by Tormin, Kolb, and Oertzen. The author draws extensively on archival sources, including the protocols of the *Rat der Volksbeauftragten* and the *Zentralrat*, most of which were recently published in English, the minutes of the conferences of state secretaries, and the personal papers of men like Groener and Eugen Schiffer. Presented in clear although undistinguished prose, his subject is topically arranged and proceeds from a summary description of the Social Democratic *Reichsregierung* and its state secretaries to a more detailed critical analysis of the policies of the high civil servants, who were charged with internal, external, and economic affairs, and the military, who presided over the demobilization of the German Army.

It is Elben's contention, and here he in part repeats what others have argued, that the SPD and USPD members of the *Rat der Volksbeauftragten* were too overwhelmed by Germany's unanticipated economic and political collapse, too much lacking in clear-cut political conceptualization and direction, too fearful of the perpetuation of the "revolution," and too impressed by the administrative *expertise* of the imperial civil servants and high military; and, therefore, they resorted to makeshift measures designed to restore order and ensure national unity and failed to realize the socialist program. Ebert initiated, and his fellow *Volksbeauftragte* supported, the continuation of the prerevolutionary state secretaries and chiefs of military authorities in their offices. Even though these men accepted the abolition of the monarchy, they behaved more like "verständesmäßige Republikaner" than obedient civil servants of the new regime. Elben convincingly demonstrates that with the help of the *Volksbeauftragte*, who regarded them as administrative experts rather than politicians and granted them remarkable independence even where firm political control was called for, the state secretaries and military chiefs made use of the opportunity to lead Germany away from revolution and to lessen, if not prevent, the break with the past.

Raymond College, University of the Pacific

GEORGE P. BLUM

DEUTSCHE OSTPOLITIK, 1918: VON BREST-LITOWSK BIS ZUM ENDE DES ERSTEN WELTKRIEGES. By *Winfried Baumgart*. (Munich: R. Oldenbourg Verlag, 1966. Pp. 462. DM 52.)

A BOOK of some four hundred pages on German policy toward Russia in 1918, which begins in the spring, *after* Brest-Litovsk, and ends with the break of diplomatic relations in the fall, may seem too long. It is. Parts of it are of considerable

interest, none more so than the central argument between the German military leaders, who were ready for all sorts of further ventures into Russia, from a march on Petrograd to the occupation of the Crimea as well as the Ukraine, and the German civilian authorities, whose strongest wish was for disengagement in the east. But stringing together résumés of memorandum after memorandum, dispatch after dispatch, and leavening them only with footnotes of inordinate length, frequency, and irrelevancy, does not make a narrative. Nor does a very respectable dissertation, which this was (the amount of research that went into it is prodigious) necessarily make a good book. This is a sad thought, but an instructive one.

University of California, Santa Barbara

JOACHIM REMAK

DER BAYERISCHE EPISKOPAT UND DER NATIONALSOZIALISMUS, 1930-1934. By *Ludwig Volk*. [Veröffentlichungen der Kommission für Zeitgeschichte bei der Katholischen Akademie in Bayern. Series B, Forschungen, Number 1.] (2d ed.; Mainz: Matthias-Grünwald-Verlag. 1966. Pp. xxii, 216. DM 29.50.)

AFTER World War II, the Roman Catholic Church in Germany portrayed itself as the victim of Nazi persecution and as the upholder of a conservative ideology upon which the new postwar regime could be based. Recently, this politically motivated version of events has been attacked by dramatists, journalists, and publicists. Historians are now beginning to examine in more detail the record of the Church in the face of the Nazi challenge. Guenter Lewy used diocesan archives to compile his remarkable indictment. Father Volk has chosen a narrower stage and a shorter period to gain a deeper awareness of the dilemmas of the Bavarian episcopate in the difficult years of the Nazi rise to power.

The Roman Catholic hierarchy in 1933 consisted of men who had been born or brought up during Bismarck's Kulturkampf. It was axiomatic to them to seek to escape from the religious ghetto of those years by identification with the popular movements of the day. Since they were inoculated against sympathy with the Left Wing, it became increasingly natural that bishops, priests, and congregations moved steadily to the Right, condemning the republic, fondly romanticizing the monarchy, and yearning for an authoritarian pattern of government under which all of the post-1918 problems would be miraculously solved. Volk does not seek to lay the blame either on the bishops, amongst whom wide divergences appeared, nor on the laity, though he makes it clear that the hierarchy disliked intensely the attempts of the Center party to come to terms with the so-called "godless" Social Democrats. On the other side, the readiness of the laity to ignore the cautious warnings of the bishops against inflamed nationalism presented the hierarchy with a dilemma, only accentuated after the Nazi take-over of power. Even while Nazi anticlerical rabble rousers instigated a reign of terror in many parts of Bavaria, the bishops perforce believed that power would make the Nazis responsible and hoped that the ideological excesses of the past would be abandoned. Hitler's own actions and speeches were certainly designed to maintain this illusion. The support of the Bavarian bishops for the concordat in July 1933 arose out of the confusion of events. They hoped that the signing of such a treaty would restore law and order in the parishes, would enable the Catholics

once again to adopt a positive attitude to the state, and would provide the necessary national unity to check the danger of Communism. Volk makes it clear that the bishops were on the whole more cautious in their support of Nazism than the generality of their flocks. But he also makes clear that, even after the atrocities of the Röhm *Putzsch* and the murder of numerous prominent Catholic laymen, the hierarchy, because of wishful thinking, was unwilling to mobilize churchmen to stand up to Nazi tyranny. The illusions about Hitler's character were only slowly abandoned. By then it was too late for the Catholic Church to save Germany from ruin.

University of British Columbia

J. S. CONWAY

STAATSMÄNNER UND DIPLOMATEN BEI HITLER: VERTRAULICHE AUFZEICHNUNGEN ÜBER UNTERREDUNGEN MIT VERTRETERN DES AUSLANDES 1939-1941. Edited and explained by *Andreas Hillgruber*. (Frankfurt am Main: Bernard & Graefe Verlag für Wehrwesen. 1967. Pp. 699. DM 58.)

ADOLF Hitler had unyielding opinions on virtually all topics, but he seemed capable of expressing them only through oral communications before live audiences. He kept no diary; nor did he reveal private thoughts in personal letters. To be sure, Hitler had "private" opinions and ideas that he carefully hid from public view, but the intimacy of even these secret thoughts was relative: the difference between Hitler's public and private utterances lay in the size and selectivity of the audience that heard him. It is for this reason that the previously published "table-talk" books are such important sources for Hitler's private life and thoughts. In a sense this edition of the confidential minutes of Hitler's conversations with foreign diplomats and statesmen during the years 1939 to 1941 constitutes another volume in the "table-talk" series.

The book covers years of triumph for Hitler, and the personality that emerges from these pages is the familiar arrogant, confident, and omniscient conqueror of Europe. The thought that Russia might survive beyond September 1941 is "laughable." His personal experiences are a sufficient basis for decisions that involve entire societies. He does not fear Americans because he met and appraised them as soldiers in 1918.

The book is well edited. A brief introduction puts the period in perspective, and each document is preceded by a prefatory note as well. Footnotes throughout the book supply numerous bibliographic and biographic data and correct many of Hitler's more outrageous factual errors. The index to persons is adequate, though, regrettably, there is no subject index. Yet, having said all this, one must ask the more basic question of whether the book needed to be published at all. Of the ninety-eight minutes assembled in this volume, only twelve were not previously available in published form; eighty-six have already been printed in the *Akten zur deutschen auswärtigen Politik* series (the English edition being the *Documents on German Foreign Policy*); and one appeared among the printed *IMT* documents. (Hillgruber gives a full citation of each document's location in the *ADAP* or *IMT* volumes.) Moreover, seven of the remaining twelve are records of mere courtesy calls. Only the remaining five are tantalizing additions. These

are very fragmentary portrayals of conversations between Hitler and Balkan, Japanese, and Spanish diplomats shortly after Germany attacked the Soviet Union. The originals were badly burned, but even these fragments reveal a Hitler who was mentally master of the world. It is as though the attack on Russia gave him a great sense of release—he had achieved his life purpose and could now devote himself to future planning on a global scale. Nevertheless, it does seem legitimate to wonder if these new documents and Professor Hillgruber's very helpful notes throughout the book are worth its not inconsiderable price.

Syracuse University

DIETRICH ORLOW

HITLEROWSKIE PRZESIEDLENIA LUDNOŚCI NIEMIECKIEJ W DOBIE II WOJNY ŚWIATOWEJ [Nazi Resettlements of the German People during World War II]. By *Janusz Sobczak*. [Studium Niemcoznawcze Instytutu Zachodniego, Number 11.] (Poznań: Instytut Zachodni. 1966. Pp. 372. Zł. 50.)
 BHE: ZACHODNIONIEMIECKA PARTIA PRZESIEDLEŃCÓW [BHE: Wes: Germany's Refugee Party]. By *Antoni Władysław Walczak*. [Studium Niemcoznawcze Instytutu Zachodniego, Number 12.] (Poznań: Instytut Zachodni. 1967. Pp. 382. Zł. 55.)

THE Western Institute in Poznań has, since its inception in 1945, specialized in studies of German-Polish relations and of German policy toward Eastern Europe. The study by Janusz Sobczak deals with Nazi attempts to resettle Germans from the Baltic States, the USSR, northern Yugoslavia, Bessarabia, and the Upper Adige particularly in western Poland. The final chapters deal with the evacuation and flight of Germans from Eastern Europe until April 1945. The author has based his work on a broad selection of published material as well as on certain surviving Nazi archives in East Germany and Poland, and the book is a valuable contribution to our knowledge of population changes in Eastern Europe. The author convincingly demonstrates two points: First, Hitler used German minorities when it suited him to justify territorial claims, as in Poland and Czechoslovakia; and, when German minorities could no longer serve his aims, special Nazi organizations persuaded them to leave their homes, or moved them forcibly, either to Germany or to western Poland in order to Germanize this long-disputed territory. Second, the study conclusively shows that neither *Reich* nor East European Germans sufficed to fill the new German *Lebensraum* in the east.

Antoni Walczak has written a study of the West German refugee party that existed from 1950 to 1961 under the name first of the *Block der Heimatvertriebenen und Entrechteten*, and then of the *Gesamtdeutscher Block*. It is clear from this study that the party never played any significant role in German politics and that it demonstrated the failure of basing a political organization on a group whose specific interests soon became lost in the process of adjustment to their new home. It is interesting to note that the territories where the party had its greatest support are the same as those that have recently given the largest number of adherents to the new Neo-Nazi party, that is, in Schleswig-Holstein and Bavaria, territories with the greatest concentration of German refugees from Eastern Europe.

Both books have short Russian- and English-language summaries. It is a pity that the English style, though intelligible, is so awkward. Perhaps the Western Institute has difficulties in obtaining adequately trained translators in Poland, but, if so, it could surely have the translations done abroad. The time and effort involved in these short summaries would be negligible for a person fluent in both languages.

University of Kansas

ANNA M. CIENCIALA

ROLNICTWO W GOSPODARCE NIEMIEC ZACHODNICH [Agriculture in the West German Economy]. By *Wacław Radkiewicz*. [Prace Instytutu Zachodniego, Number 39.] (Poznań: Instytut Zachodni. 1966. Pp. 209. Zł. 37.)

THIS monograph is devoted to the study of agriculture in the German Federal Republic after World War II. Economic and historical analysis has been related to theoretical considerations. The author's Marxist assumptions permeate his observations of empirical phenomena; they led the author to assess the contradictions visible in German agriculture as well as in the whole economic system. This "scientific foresight" also allowed the author to foresee the increase of such contradictions with the full integration of the republic within the Common Market. Such contradictions seem to the author unavoidable in the capitalistic economies in general.

The positive observations suggest that the main task of agriculture in the republic after 1945 was a quick reconstruction of its productive capabilities, the intensification of farming, and an increase in supplies of agricultural commodities. These objectives have been achieved in a brief period of time. In contrast to the general economic revival of the country, the place of agriculture has shrunk, its share in the national income declining from 10.3 per cent in 1950 to only 5.3 per cent in 1965.

Together with such phenomena, the prospects of profitable agriculture relatively dropped. "Relatively" means that the author noticed a basic discrepancy between profit in agriculture as related to profit in industry and commerce. The scissors of prices, an insurmountable contradiction in all capitalistic systems, grew significantly, especially after the "tractor revolution" and the technical improvements that increased agricultural productivity and output. There is no parity of income, in spite of governmental intervention and protection; agriculture has become dependent on industrial monopolies and commercial capital. Although the prevailing vertical submission of agriculture to the monopolies or commercial capital does not need concentration of landholdings, such concentration is effected by other factors and has manifested deep changes in the agricultural structure. Small and medium farms are disappearing, and large farms are becoming capitalistic. Farmers lost their economic independence in the fall of the concentrated capital engaged in agricultural processing and distribution, and small and medium farms have fallen into a "relative poverty" situation. The sharp contradictions are manifested here. The class struggle cannot be overcome in the existing capitalistic system. The old conflict, "bourgeoisie versus proletariat,"

has been replaced by the same conflict in a new form: "monopolies versus peasants"!

This book only surveys selected issues. The "relative poverty" of German peasants should be shown in relation to their ability to satisfy their needs from their incomes, while governmental intervention, the "green programs," and credit matters have been left without further explanation. This useful book is well written, and it is supplied with twenty-two pages of statistical tables and diagrams. The bibliography is fairly comprehensive, although mostly Polish and German. The work was issued as a paperback with Russian and English summaries.

Wisconsin State University, Stevens Point

WACŁAW W. SOROKA

Erratum: Omission of one word in the printing of the review of M. Botzenhart, *Metternichs Pariser Botschafterzeit* [*AHR*, LXXIII (Dec. 1967), 530], unfortunately completely reversed the meaning of the sentence and, indeed, of the review. The sentence as it now stands reads: "Indeed, Metternich's ambassadorial reports are not distorted by his natural preference of the regime; they express a thoroughly pragmatic approach to the new empire and its institutions." Clearly, the word "old" is missing before "regime."

ÖSTERREICH 1938-1945: IM SPIEGEL DER NS-AKTEN. By *Karl Stadler*. [Das einsame Gewissen, Number 3.] (Vienna: Verlag Herold. 1966. Pp. 427. 432 Sch.)

THIS is a brave attempt to present a picture of that powerful but elusive political force, public opinion, in Austria from the *Anschluss* to the end of the war. As sources for the ideas and behavior of the "average man," the author has used the Gestapo *Tagesrapporte*, the SD *Lageberichte*, and other National Socialist archives. His focus is on the growth of opposition to the Nazi regime among the many diverse, often mutually hostile, social and geographical groups in Austrian society.

Stadler divides the Austrian population into actual or nominal Nazis, non-Nazis, and anti-Nazis, of which the middle group constituted the great majority. This majority was originally ambivalent in attitude toward the Nazis and German rule, he believes, but gradually began to oppose it. The Nazi archives, he shows, contain evidence of innumerable acts of civil disobedience, unreliability, and positive resistance, not only by anti-Nazis but by the non-Nazi majority.

He emphasizes that in view of the Nazi state's demand for total obedience, every sort of opposition in the Third Reich must be regarded as a form of resistance, even to not entering the party under certain circumstances. He admits that in order to assess how much any given individual in this sense "resisted" would require exact knowledge of the amount of compulsion exercised on him by the Nazis and the penalty for refusal—an almost impossible feat in most cases—but he illuminates, if he cannot solve, this problem of understanding the nature and extent of "resistance" in totalitarian states with a wealth of examples covering a wide range of social groups throughout the seven years during which Austria was part of the Third Reich. He shows how varied were the motives of

these heterogeneous groups: workers might resist the regime because it exploited labor (but welcome, as Karl Bednarik has pointed out, other aspects of the regime and "see no connection between the fun of glider training and an uncle in Buchenwald"), peasants might oppose Nazi hostility to religion, and so forth. Stadler notes how the combined opposition to Nazism of these groups became a struggle for national liberation as well.

Though the author comes to no radical new conclusions, and his book lacks firm organization—it is constructed in a curious rambling, rather amateurish form—it offers a valuable contribution to knowledge of a still-obscure period in Austrian history and of conditions of life in Nazi Germany.

Queens College

ANDREW G. WHITESIDE

ECONOMIA E SOCIETÀ NELLA CALABRIA DEL CINQUECENTO.

By *Giuseppe Galasso*. [Università di Napoli, Seminario di Storia Medioevale e Moderna, Number 2.] (Naples: L'Arte Tipografica. [1967.] Pp. ix, 480. L. 6,000.)

It is the thesis of this book that in the sixteenth century there was considerable vitality in the economic life of Calabria stimulated primarily by outside merchants seeking silk and other products in this backward area where prices were not keeping up with the inflation in the Western European markets at the time. The author supports his case with a careful and detailed survey of all economic activity in the region up to the early seventeenth century, when the "boom" came to an end as a result of withdrawal of the external stimulus and the oppressive fiscal policies of the government, not to mention the inherent economic weakness of the area. He assumes throughout that this economic activity momentarily introduced a new dynamic in Calabrian society that could have set it on a more progressive course, but he shows how instead there was a strengthening of the grip of the feudal nobility over local areas so that by the early seventeenth century the feudal regime was more oppressive than ever, reversing the trend of the late Middle Ages and setting the tone of modern Calabrian history.

The analysis is based chiefly on archival materials, although, needless to say, the documentation for an economy so rural and relatively undeveloped precludes anything but an impressionistic account. It is perhaps for the same reason that there is no analysis in depth of Calabrian society below the level of the nobility, and many of the author's comments about that society, however reasonable, are entirely gratuitous. The usefulness of the book, in fact, is considerably limited by the author's failure to discuss the problems of his sources, and the book will be disappointing to anyone seeking a methodology for regional studies. The book develops, nevertheless, various themes in the history of southern Italy in general (for instance, the vigor of Genoese enterprise in exploiting the economic possibilities of the region, the seriousness of Turkish raids throughout the century in disrupting life all along the considerable coastline, the tragic failure of the viceregal government to follow a more enlightened fiscal policy and to forestall the refeudalization of southern Italy, the impressive growth of population during the century, and the increase of social unrest marked by banditry, popular uprisings, and an accelerated internal migration). We will need more regional

studies like this in order to piece together the historical puzzle of the development of the Mezzogiorno.

Kent State University

RICHARD A. GOLDTHWAITE

INDUSTRIA E COMMERCIO IN LIGURIA NELL'ETÀ DEL RISORGIMENTO (1700-1861). By *Luigi Bulferetti* and *Claudio Costantini*. [Studi e ricerche di storia economica italiana nell'età del Risorgimento.] (Milan: Banca Commerciale Italiana. 1966. Pp. 564.)

ITALIAN historians were late in showing an active interest in the economic evolution of their country in modern times. They failed to do so long after Raffaele Ciasca, in his *L'origine del programma per "l'opinione nazionale italiana" del 1847-48* (1916), had pointed out how largely economic considerations figured in the thought and hopes of the leaders of the *Risorgimento* and how abundant and accessible were the materials in which the relationships of these to contemporary economic changes could be explored. The present volume and the series in which it appears invite attention to the progress now being made in establishing a solid groundwork of fact for the economic history of modern Italy.

The series, in which this volume is the seventh, was directed by Federico Chabod until his death in 1958, and the authors are among the most distinguished in recent Italian historiography: Luzzatto, Berengo, Villani, Romano, Dal Pane, Caizzi, not to mention Luigi Bulferetti, coauthor of the present volume. A comparably weighty series, the "Archivio economico dell'unificazione italiana," edited by Professor Carlo Cipolla, is in course of publication. It will seem remarkable to Americans that a scholars' compilation such as the "Studi e ricerche" is being published by the *Banca Commerciale Italiana*. But also to a bank historiography owes one of the first and most important contributions to the economic history of modern Italy, the volume of scholarly studies sponsored by the Savings Bank of Lombardy to celebrate its hundredth anniversary in 1923.

What the authors of the present volume offer is "a stimulus, we trust, and a first contribution to . . . systematic investigation of the economic and social history of modern Liguria," that is, of Genoa, and the contiguous territory that was under its dominion in 1700. They have drawn their information from contemporary publications and from the documents that are open to research, chiefly concentrated in the Archives of State in Genoa and in the port towns of the Italian Riviera. This information they present in detail, with critical appreciation of the limitations that its scope and content impose, not only on the comprehensiveness of their account, but also on the answers they can give to questions that historians will ask, and of whose importance the authors are well aware. At the same time they point out the corrections that their findings seem to require or justify in conclusions presented in previous publications, monographic or general, bearing on the subject. It is obvious that this is a book primarily for specialists. Others will find it profitable only if they bring to it a knowledge of economic history and of the history of Italy, which the authors, in their present undertaking, have to assume that their readers already share with them.

Baltimore, Maryland

KENT ROBERTS GREENFIELD

THE COMING OF THE ITALIAN-ETHIOPIAN WAR. By *George W. Baer*. (Cambridge, Mass.: Harvard University Press. 1967. Pp. viii, 404. \$9.50.)

SCHOLARS can be grateful to Professor Baer for his enlightening, well-documented, and well-written book on the events that, starting with a border incident between Ethiopians and Italians on December 5, 1934, led to the massive invasion of Ethiopia by Italian troops on October 3, 1935. The author, a former Rhodes scholar, rightly points out the tragic impact of the last aggression by a European power against independent Africans: the defeat (for the time being) of the generous ideals embodied in the League of Nations; the forging of closer links between the two major European fascist powers, Italy and Germany; the appeasement practiced by Great Britain and France leading straight to World War II.

The book is based on more extensive material than that used by Salvemini in his *Prelude to World War II*; nearly a thousand footnotes testify to diligent research; the variety of titles in the twenty-two-page bibliography shows how the author familiarized himself with the complex scene of which the Italian-Ethiopian conflict was an episode. The recent and not so recent background is disposed of in the first two chapters, about one tenth of the book. The other eleven chapters, organized chronologically, contain a lucid, valuable, and detailed analysis of the events and a critical evaluation of facts and people. The pedantic reader can find blemishes: Giovanni Gentile is dismissed as a "Fascist propagandist," but he was important because he was the second-ranking Italian philosopher. Both De Bono and Badoglio were professional soldiers and Fascist functionaries; they were not alike in mental capacity. The Italian *grano* is not grain but specifically wheat. The term "Arditi" does not apply to the troops sent to the Brenner Pass.

An observation should be made on a different level. It is conventional to ascribe only sordid motives to the actions of British and French leaders. In the late 1960's, especially, one should not disregard the fact that peace was as deep and sincere a concern for large sections (actually the overwhelming majority) of the British and French publics and of their governmental representatives as it is for Americans today. To many, in 1935, localization of the conflict in East Africa and peace with dishonor in Europe were preferable to the ravages of war at home or near home. The appeasers were in error, but their alternative was readiness to face a major war between European powers in the Mediterranean.

Smith College

M. SALVADORI

STUDIA NAD DZIEJAMI WIELKOPOLSKI [Studies on the History of Great Poland Province]. Volume III, 1890-1914. By *Witold Jakóbczyk*. [Poznańskie Towarzystwo Przyjaciół Nauk, Wydział Historii i Nauk Społecznych, Prace Komisji Historycznej, Volume XXI, Part 3.] (Poznań: Praca Wydana z Zasiłku Prezydium Wojewódzkiej Rady, Narodowej w Poznaniu. 1967. Pp. 254. Zł. 62.)

THE author is professor of the history of Poland in the nineteenth and twentieth centuries at the University of Poznań. The present volume concludes his three-

volume series devoted to that conspicuous phenomenon of Polish history known as "organic work." Volume I, covering the years 1815-1850, appeared in 1951, and Volume II, on the period 1850-1890, was published in 1959. The term "organic work" (*praca organiczna*), first employed in the 1830's, denotes the great movement of self-help organized by the Poles in the nineteenth century and designed to strengthen and elevate the Polish nation economically, socially, and culturally, in the face of pressure upon their national life from the partitioning powers. The movement developed in all three parts of partitioned Poland, but attained its classic forms in the Prussian-occupied segment, and it is with the Prussian scene that Jakóbczyk is concerned. Jakóbczyk, whose first works were published in the mid-thirties, has devoted a lifetime to the study of Prussian Poland, and especially of the "organic work" in that region.

The author invokes the ghost of Lenin in the second sentence of the preface, and having thus disposed of customary courtesies, feels free to pursue the historian's craft without ideological extravagance. His account is reinforced by recourse to archival sources and contemporary Polish newspapers. Each chapter deals with one organization or group of organizations through which "organic work" was carried on, among them peasant groups, women's clubs, the Sokol gymnastic society, and various cultural organizations of burghers. There is also a chapter on the political movements of the "propertied classes." The author's tone is sober and reflective. He underscores the character of "organic work" as a movement of middle and higher social classes. He muses on his own early idealization of this movement; at the same time he recognizes the contributions the "propertied classes" made, by this movement, to Polish national life and writes with sympathy about their achievement. His account offers one more illustration, if such be needed, of the unique ability of the Polish people, propertied or otherwise, to survive under conditions of adversity of the kind few other nations have ever been called upon to face.

University of British Columbia

STANLEY Z. PECH

THE OCCUPATION OF CHIOS BY THE GERMANS AND THEIR ADMINISTRATION OF THE ISLAND: DESCRIBED IN CONTEMPORARY DOCUMENTS. By *Philip P. Argenti*. (New York: Cambridge University Press. 1966. Pp. xiv, 375. \$22.50.)

Dr. Argenti, who is cultural counselor to the Greek embassy in London, is the author, coauthor, or editor of some eighteen volumes on the history of the island of Chios. The subject matter of these volumes ranges from folklore and costumes to the Turkish conquest of 1566, the Turkish massacre of 1822, and the liberation of 1912. In composing the present work Argenti no doubt had in mind his *The Occupation of Chios by the Venetians 1694* (1935) and *The Occupation of Chios by the Genoese, 1346-1566* (1958).

The present work is roughly one-third history—presented in the most painstaking detail—and two-thirds documentation, the documents being presented in the original language, whether German, Greek, French, or English. Text and documents are supplemented by maps, plans, and photographs. This careful and

scholarly presentation is to some extent marred by a distinct royalist bias and a tendency to present events, such as the Communist rising of March 1948, without reference to their national context.

Of particular interest to those concerned with the history of World War II will be Argenti's dry and minutiose account of German occupation policy. Order was maintained by the taking of hostages (Argenti attributes, in fact, the sparseness of Chian resistance activities to concern for the fate of the hostages). The Gestapo was not represented on the island, but the interrogation methods employed by the German Army version of the American Counter Intelligence Corps by no means fell within the limits of the Geneva convention. The occupiers organized a concentration camp in a schoolhouse and fitted out a macabre dungeon in the basement of a private home. They appropriated much of the available food supply, leaving the population near starvation. The Germans did, on the other hand, permit the Swedish Red Cross to bring in food. And none of the hostages were shot.

Wayne State University

R. V. BURKS

GEHEIME REICHSSACHE: PAPST PIUS XII. HAT NICHT GESCHWIEGEN. BERICHTE—DOKUMENTE—AKTEN. Compiled by *Jenő Levai*. With a foreword and a conclusion by *Robert M. W. Kempner*. (Cologne: Verlag Wort und Werk GmbH. 1966. Pp. 144, 13 plates. DM 17.80.)

THE debate over Pius XII's attitude toward Hitler and the Jews, set off by Rolf Hochhuth's play, *The Deputy*, in 1963, shows no sign of abating. The present work, which deals with the activities of the Vatican and the Catholic hierarchy in Hungary on behalf of the Hungarian Jews, is intended as a reply to Hochhuth and aims to show that Pius XII had not remained silent.

Mr. Levai, who, according to the book jacket, is a Hungarian historian and an authority on matters pertaining to the persecution of Jews in Hungary, assures us that "the protection of the Vatican saved many thousands of lives from German deportations. . . . In this case the Pope had spoken out. His directives had saved thousands of lives. This is the historic truth." It may well be the historic truth, but to accept what is presented here is to rely to a large extent on the author's word. His methods of presentation do not inspire confidence.

For the "most accurate" documentation Levai refers the reader to his earlier book, *The Black Book on the Martyrdom of Hungarian Jews* (1948), but fails to provide cross references to it. In at least one instance—the saving of lives through the intervention of the Pope—the claims in the *Black Book* are considerably less extensive than in the present one. In a note on sources, the author states that sources for quotations are given in footnotes; this he does only occasionally and in the most casual fashion. Worse, he refers to meetings and conversations, opinions and statements without citing supporting evidence. The text of those documents that are printed is in many instances incomplete and without indication of what was omitted. If, instead of meaningless photographs, Levai had used facsimiles of important Hungarian and Vatican documents, as was done with a few Ger-

man ones, his statements and conclusions would have been far more acceptable.

The problem of papal diplomacy toward Germany during the Second World War is far too important to be treated emotionally and journalistically. A scholarly study, based on documentary evidence, is still needed.

Washington, D. C.

GEORGE O. KENT

THE NEW RUMANIA: FROM PEOPLE'S DEMOCRACY TO SOCIALIST REPUBLIC. By *Stephen Fischer-Galati*. [Center for International Studies, Massachusetts Institute of Technology. Studies in International Communism, Number 10.] (Cambridge, Mass.: M.I.T. Press, Massachusetts Institute of Technology, 1967. Pp. xi, 126. \$6.00.)

PERHAPS the first book-length effort to deal with the Rumanian national deviation, this volume is essentially concerned with tactics and strategy. The economic issues are left to a forthcoming volume by Michael Montias in the same MIT series, while the interiorization of the power of the regime is only sketched in, with minimal reference to the change in minority policy and none at all to the rehabilitation of pre-1944 national culture. Professor Fischer-Galati's main concern, and a proper one, is to explain, in a technical political sense, how the Rumanian Communist regime managed to escape Soviet domination and establish its own independence.

The success of the Rumanians, Fischer-Galati argues, was owing to a variety of elements, of which the "exceptional political acumen, courage and tenacity" of Gheorghe Gheorghiu-Dej was not the least important. Another was a basic shift in the constellation of the Great Powers: the Sino-Soviet schism, with its undermining of Moscow's control over international Communism, was conjoined with the Cuban confrontation, which reduced Moscow's power in the larger world. In the course of the author's presentation, a number of new (and sometimes startling) interpretive positions emerge. Thus, for example, he argues that the purge of Constantinescu and Chisinevski in 1957 amounted to the removal of Khrushchev's principal political allies within the regime, while the withdrawal of the last Soviet divisions from Rumania in 1958 he presents as the consequence of a Rumanian initiative which succeeded in part because of Chinese backing.

Fischer-Galati's analysis is based on extensive, careful, and frequently brilliant exegesis of official documents in the original Rumanian, of key speeches, editorials, communiqués, commemorative greetings, and the like. His argument is, in general, strong and convincing, but at times (in my opinion) he presses his materials too far, asking the reader to content himself with a casual "it is now known" or "it is now believed." There is a hint on the dust jacket that during the course of his research the author had interviews with Rumanian officials, but the footnotes bear no reference to any other than publicly available documentary information. Fischer-Galati's book represents, nonetheless, a significant addition to our knowledge and understanding of the Rumanian national deviation.

Wayne State University

R. V. BURKS

RUSSKAIA ISTORIOGRAFIIA XVIII VEKA [Russian Historiography in the 18th Century]. Part 2. By S. L. Peshtich. [Leningradskii Ordena Lenina Gosudarstvennyi Universitet, Imena A. A. Zhdanova.] ([Leningrad:] Izdatel'stvo Leningradskogo Universiteta. 1965. Pp. 342.)

S. L. PESHTICH's second volume of his three-volume studies of Russian historiography of the eighteenth century is a welcome addition in this field and shows the growing independence and sophistication of Soviet scholarship. One has only to recall the excesses of Tikhomirov in the late 1940's, when the party line dictated that German scholars living in eighteenth-century Russia were forerunners of fascist imperialism, to rejoice at the great distance back to sanity now traversed.

After N. L. Rubinshtein's excellent survey on Russian historiography in 1941, this field remained a swamp until well after Stalin's death. The collective multi-volume *Ocherki Istorii Russkoi Istorii* (1955) could still devote more than twice the space to Alexander Radishchev—certainly a great prophet, but no historian—than it gave to Shcherbatov and Boltin combined, both historians of distinction. Cherepnin's survey of Russian historiography up to the nineteenth century (1957) showed rising standards, but was merely classroom lectures. Peshtich's work shows an impressive familiarity with sources and bibliography and draws sensibly, without polemics or condescension, on pre-Soviet Russian historians. This volume traces the growth of historiography in Russia in the second half of the eighteenth century. The opening chapter sets forth the general state of historical knowledge in Russia from about 1750 to the French Revolution, with useful summaries of histories of science, encyclopedias, and lexicons. The seminal importance of *Monthly Compositions* (1755–64) where so many historical works were first presented to the public in that “marvelous decade” is well documented. The pages on the interaction between historiography and the fine arts are original and lively.

The second chapter treats the theoretical foundations of historical knowledge—“Historians on History.” The remaining chapters analyze the works of Tatishchev, Lomonosov, G. F. Miller (Müller), Schlözer, Emin, Catherine II (!), Rychkov, and two “bourgeois” historians, Krestinin and Chulkov. Shcherbatov, whom Peshtich considers the most formidable historian of the century, and Boltin, whom the author calls the most able philosopher of Russian history in the period, are left for detailed treatment until Volume III, although each is treated briefly in the second chapter.

The chapters on individual historians are uniformly good, but the one on Tatishchev presents the most new material. Peshtich, on the basis of Protasov's researches and his own, challenges the traditional view of this father of Russian history as an apologist for unlimited absolutism. While Peshtich rightly insists on Tatishchev's appreciation of linguistic tools in writing the history of peoples, one needs to go to Hans Rogger to find the limitations of this approach when naïvely used; one would not learn from Peshtich, for example, that Tatishchev held the Slavs to be descendants of the Amazons or *Alazony*, a Slav tribe whose Greek name means glorious (*slavny*). But Peshtich's indulgence with regard to methodological inadequacies certainly is not owing to chauvinism, which he roundly castigates whether it is a question of eighteenth-century or Soviet falsi-

fiers of the date of the earliest Russian printing press, or of ignoring Tatishchev's high appreciation of the German, Bayer.

Peshtich also takes an objective stand in the long controversy over the relative merits of Lomonosov and Miller, although he modestly avers that such a stand is yet to come. In passing he alludes caustically to the "peculiar Neo-Slavophile point of view in Soviet literature [on this controversy] during the years of the cult of personality," a euphemism for the rampant nationalistic self-adoration under Stalin's terror. At last it can be written that "Normanism" in Russian historiography was not simply a self-serving plot of German-Russian scholars and that Miller, the German, was a better historian than his opponents and a man of praiseworthy public spirit. But it seems old-style forcing of the data to portray Tatishchev as "the first historian of the Russian peasantry," this after showing that his contribution in this field was simply to trace peasant rebellions to heresy.

Miller and Schlözer are praised for introducing critical methods into Russian historiography, but the reader is not given proof. Here again, as in the listing of influences on Tatishchev, there is an abstract quality in the presentation that contrasts with the usually concrete and careful exposition of actual views.

An unexpected dividend of this book is the half chapter on Catherine the Great's historical writings. She took time during a quarter century of masterful rule to write a volume of history and to participate in the riposte to Chappe d'Aueroche, *Antidote*, which Peshtich characterizes as a link between Tatishchev on the one hand and Shcherbatov and Boltin on the other—much too generous a view and quite amazing in a Soviet historian.

Peshtich's work has no major new insights and is not a rival to Miliukov's chef-d'œuvre on Russian historiography written some seventy years ago. But it is full of sensible judgments, meticulous accuracy, and impressive scholarship. Its consistent scrupulousness, civility, and high competence augur well for the future of Soviet scholarship at a time when the harassment and arrest of writers and the vilification of a prominent recent *émigrée* give cause for concern.

Queens College

ALLEN MCCONNELL

DANILEVSKY: A RUSSIAN TOTALITARIAN PHILOSOPHER. By Robert E. MacMaster. [Russian Research Center Studies, Number 53.] (Cambridge, Mass.: Harvard University Press. 1967. Pp. ix, 368. \$7.95.)

ONE cannot but admire the tenacity of a student in pursuit of a suggestion made to him by a beloved professor (who died in 1959) that he study Danilevsky, "squeezing the scanty data very hard and placing him in his time." MacMaster has attempted to pursue "a somewhat new kind of historical research, using comparative historical, social scientific, and philosophical-psychological (mainly phenomenological) concepts quite extensively" and to write "a biographical history of the modern Russian radical mind, both romantic . . . and totalitarian." In the process he has steeped himself in pseudoscientific jargon: "Voluntaristic political action (always restricted to the deeds of the moment and never directed to the millennium itself) is in the totalitarian mind simply the most crucial human correlative of the self-movement of the All toward a beatific equilibrium."

Admiration for MacMaster's ambitious effort cannot be extended to include

its product. It simply is not possible to make a silk purse out of a sow's ear. N. I. Danilevsky (1822-1885), acknowledges MacMaster, was simply "a crank," "really a very ordinary intellectual," best known for his book, *Russia and Europe* . . . (1869). This book was "a distinctly amateurish work, full of loose ends, weak in unity and coherence. . . ." Though its "doctrine was a dangerous and evil one," its "messianism is muted and unclear." Its influence was nil, despite its "discovery" by Spengler and Toynbee. Yet Danilevsky "was a forerunner of the Bolsheviks . . . not of the Nazis"; just how this was so is not made clear. Danilevsky was a "bureaucrat," with scientific training, employed by the government to study agricultural and piscatory problems; "except perhaps for his work on the fisheries, modern Russian . . . history would scarcely have missed Danilevsky at all."

Yet, says MacMaster, "I have come to like him as a human being." It is presumably for this reason that he painstakingly puts together the fragmentary evidence about Danilevsky's deep interest in Fourierism, about the influence of Slavophilism, about his peripheral role in Pan-Slavism, and about his "tragic" transition from a "humanistic radical" to a "totalitarian" one.

It is curious that MacMaster's wide-ranging quest for significance to be squeezed out of Danilevsky permits him to preface his chapter on "Panslavism" with a discussion of Slavophilism, with no reference to its vital Czech origin. Nor does he, in his ideogenealogical quest, cast his eyes back to Krizhanich, whose seventeenth-century manuscript was first published about ten years before *Russia and Europe*; presumably Danilevsky was unaware of it, but reference to it might have helped MacMaster to provide "useful insight . . . into the human meanings" of Danilevsky's thought.

The book is equipped with a list of Danilevsky's writings, extensive notes, and a serviceable index.

Brooklyn College

JESSE D. CLARKSON

PRISOEDINENIE SREDNEI AZII K ROSSII (60-90-E GODY XIX V.)
[The Annexation of Central Asia to Russia (1860's to 1890's)]. By N. A. Khalfin. [Akademiia Nauk SSR, Institut Narodov Azii.] (Moscow: Izdatel'stvo "Nauka." 1965. Pp. 466.)

In this able though highly tendentious analysis of Russian foreign policy regarding Central Asia from the end of the Crimean War until accommodation with Great Britain over the Pamir region the author discusses the topic in the light of Russian internal conditions, especially economic matters, and the problem of Anglo-Russian rivalry in Central Asia. Other researchers will find his lengthy discussion of pre-1917 Russian and Western European as well as Soviet printed and archival sources useful, but his treatment of modern Western literature in this field is distorted and incomplete.

Khalfin is naturally at his best when dealing with the Russian side of events. Quoting from previously unpublished documents he provides new facts concerning behind-the-scenes planning of the military assaults in Central Asia and differences within government circles over tasks and aims.

His chapters on British efforts in Central Asia are, on the other hand, weak

and contrived. In the spirit of his previous works (in Russian), for example, *Failure of British Aggression in Afghanistan in the 19th and Early 20th Centuries* (1959) and *The Formation and Break-up of the British Colonial Empire* (1961), he finds it convenient to retain the outmoded doctrine that Britain intended penetration and dominance of areas eventually seized by Russia. This seizure, he takes pains to indicate, was "annexation" (a euphemism now preferred by Soviet historians to "conquest") based on the willingness of a few Central Asians of the middle or well-to-do groups to come under Russian rule. Passing judgment on yesterday in the light of today's concepts, he condemns "Tsarist colonizers," but considers the imposition of Russian rule "progressive" because it speeded socialism in Central Asia. Rightfully criticizing the legend of a "Russian menace to India," formerly propagated in England, he thus dutifully repeats the official legends of modern Soviet historiography.

There are bibliographical footnotes and an extensive bibliography, but no maps or index.

Queen's University

RICHARD A. PIERCE

LENIN: UNBEKANNTE BRIEFE, 1912-1914. Edited by *Leonhard Haas*. ([Einsiedeln:] Benziger Verlag, 1967. Pp. 156. 18.80 fr. S.)

BEFORE the outbreak of the First World War, Lenin carried on an active correspondence with two of his comrades in Bern, F. N. Samoilov and G. L. Shklovsky. Some two dozen of his letters subsequently found their way into the Swiss Federal Archives, of which Dr. Haas is the director, and are published now for the first time in this compact volume. The letters themselves occupy only twenty-six pages of the book; an introduction by the editor, a digest of the letters' contents, German translations and photographic copies of the Russian originals, explanatory notes, and indexes take up the rest.

The letters deal with two subjects. The first is the health of Samoilov, a Bolshevik member of the Fourth Duma, who had gone to Bern to seek treatment for a persistent nervous disorder. Lenin shows deep personal concern for his ailing comrade. That this, however, was in no small part inspired by a desire to see Samoilov return to his parliamentary duties as soon as possible is evident from Lenin's remark to Shklovsky that "we are in devilish need of a deputy who is completely healthy." The second subject is the so-called "Schmidt Legacy," a large fortune over which the Bolsheviks and Mensheviks were vying for possession. Though the letters show Lenin's tireless efforts to lay hands on the inheritance, unfortunately they present little new information on the affair. We remain ignorant of, among other things, the precise amount of money involved and its ultimate fate. The whole story needs further elucidation, which, one hopes, will eventually be provided by an examination of unpublished materials in the International Institute of Social History, the archives of the Second International, and the Nicolaevsky Archive of the Hoover Library, to which the editor of this volume apparently has not had access.

A modest addition to the existing body of Leniniana, Haas's book contains nothing that will alter the prevailing picture of Lenin and his party on the eve of the war. Yet no one will blame the editor for his tendency to exaggerate the

significance of this new batch of letters. For it may justly be argued that every scrap of Lenin's correspondence possesses intrinsic importance, especially in this fiftieth anniversary year of the Russian Revolution.

Columbia University

PAUL AVRICH

THE SOVIET MILITARY AND THE COMMUNIST PARTY. By *Roman Kolkowicz*. (Princeton, N. J.: Princeton University Press. 1967. Pp. xvi, 429. \$9.00.)

IN probing the relations between the military and the Communist party of the Soviet Union (CPSU) Kolkowicz is not exactly breaking new ground, but his book represents the most thorough, up-to-date, and sophisticated treatment of this fascinating subject. Communist relations with the armed forces from 1917 to 1941 have been treated in two outstanding volumes: D. Fedotoff White, *The Growth of the Red Army* (1944), and John Erickson, *The Soviet High Command* (1962). Various aspects of the period since World War II have been covered in books and articles by Raymond Garthoff and others. Kolkowicz, however, is the first to present a general synthesis that emphasizes the period since Stalin's death. His book (earlier versions of which served as a doctoral dissertation at the University of Chicago and as a RAND study) is based on an extensive examination of books, pamphlets, newspapers, and periodicals printed in the Soviet Union, in Eastern Europe, and in the West.

As in the case of any governing elite, the CPSU must have strong, professional forces if it is to pursue its foreign policy goals, maintain a leadership role within the Communist bloc, and protect its borders. The professional military establishment, however, represents a threat to absolute party control not only because it possesses a monopoly of the means of violence but because it tends toward elitism, group autonomy, and heroic symbolism, all of which are undesirable traits according to the Communist creed. The result has been a history of conflict and tension, which has risen and subsided according to the degree of internal and external stability of the Soviet Union.

In examining this conflict Kolkowicz makes use of history, but his method is more analytical than historical. Approximately one-third of the book deals with the nature of institutional conflicts including an examination of the structure, instruments, and methods employed by the CPSU to control the military establishment. Another third deals with specific issues of conflict that developed in the period 1953-1963. Finally the rise of the new technology and its present and possible future effects are discussed. The author finds that a mild form of pluralism has grown up in Soviet society and that the role and influence of the military are likely to rise in the future.

The organization of material and the methodology employed by the author have the advantage of achieving comprehensiveness and depth of treatment that would otherwise be difficult to achieve; they have, on the other hand, the disadvantage of producing a certain amount of repetition and redundancy. Also, whether because of the methodology or the inherent difficulties of the subject, the style of writing is diffuse and graceless. The book, nevertheless, is apt to be a

landmark in the growing literature on civil-military relations in the Soviet Union.

Ohio State University

HARRY L. COLES

Near East

THE KITĀB AL-MAGHĀZĪ OF AL-WĀQIDĪ. In three volumes. Edited by Marsden Jones. (New York: Oxford University Press. 1966. Pp. xiii, 8 plates, 413; 415-871; 873-1321. \$20.20 the set.)

STUDENTS of the life and campaigns of Mohammed will welcome this full-length scholarly edition of Wāqidī's *Maghāzī*, second in time and significance only to Ibn Ishāq's *Sīrah*. The edition is based on several manuscripts, the earliest of which is dated A.H. 465/1073 A.D. The work is particularly valuable for its emphasis on chronology, methodical presentation, and the use and supplementation of earlier sources. The edition is marred by the omission of pages 369-384 of the text. Classified indexes make the work readily usable. Arabists and the Muslim world owe the editor a debt of gratitude for an arduous task well done. The promised publication of Ibn Bukair's transmission of part of the *Sīrah* and possibly an English translation of the *Maghāzī* would indeed be welcome.

The editor's brief biobibliographical sketch of Wāqidī overlooks Barmakid influence on his works and the fact that a copy of the *Maghāzī* was in their library—much as earlier the caliph Manṣūr had influenced Ibn Ishāq and placed a copy of the full *Sīrah* in his library. Seventeen years of Wāqidī's life following the fall of the Barmakids are passed over in silence, as is also Wāqidī's relationship to Ibn Ishāq's still active pupils in Iraq, one of whom possessed the original papyrus draft of the *Sīrah*. The editor's view on the degree of Wāqidī's dependence on Ibn Ishāq is not convincing, based as it is on the analysis of their respective accounts of but two episodes in their lengthy works. The absence of literal parallels in the extant abridged version of the *Sīrah*, furthermore, does not preclude Wāqidī's use of the original text of the *Sīrah*, which at that very time Ibn Hishām was busily abridging in Iraq itself. The availability of the *Sīrah* through the above channels or even through the flourishing book market that Wāqidī was known to frequent should be further explored.

Any further involvement with early Islamic historiography should also consider more recent views than those of Kramer, Wellhausen, and Horovitz, on which the editor has mainly relied. F. Rosenthal, *A History of Muslim Historiography*, Guillaume's barely mentioned introduction to his translation of the *Sīrah*, and my still more recent *Studies in Arabic Literary Papyri*, Volumes I and II, are pertinent to that field.

University of Chicago

N. ABBOTT

THE BASTARD WAR: THE MESOPOTAMIAN CAMPAIGN OF 1914-1918. By A. J. Barker. (New York: Dial Press. 1967. Pp. xiv, 449. \$7.50.)

COLONEL A. J. Barker, a professional British officer of the Indian Army, is best known in the United States for his recent *Suez: The Seven Day War*, a much-

needed account of the military side of the Suez fiasco of 1956. In *The Bastard War*, Barker has finally done justice to that classic of Near Eastern muddle in the First World War leading to the unnecessary siege and eventual surrender of Major General Charles Townshend's division at Kut-al-Imara in April 1916.

Barker attributes this resounding British failure essentially to the parsimony and incompetence of the British government of India, coupled with the exceptional logistic and climatic difficulties of Mesopotamia. Contrary to the desires of the more realistic and western front-minded British Army general staff in distant London, the "Mesopot side-show" simply grew without adequate premeditation or preparation. And as the author implies, the political ends of the Mesopotamian campaign were like Vietnam today, as ambiguous as its military means.

In a particularly interesting epilogue, Barker summarizes his basic conclusions regarding Mesopotamia in an effective contrast with the slightly antecedent British failure at Gallipoli. Of the Dardanelles campaign he writes: "Behind the charming, ineffective Hamilton and the romance of Troy lay the whole political might of Whitehall; behind Barrett and Nixon [in Mesopotamia], a collection of individuals totally unfitted to control any sort of campaign, civil or military. 'Apportioning blame and responsibility' comes easy when it lies outside one's own aegis . . . [but] Commissions inquiring into the operations of the Somme . . . or on the causes of the fall of Singapore and the ill-fated Suez venture in 1956 could have resulted in disconcerting disclosures whose reverberations . . . might well have rocked the seats of politicians and service chiefs alike."

In other words, since 1916 the Mesopotamian campaign has been appraised as it was conducted at the time, namely as a dispensable side show. The bastard war thus constituted an ideal scapegoat campaign with which to dissipate the natural concern of the British public over far larger military issues and errors. Adequate maps, fine photographs, and an excellent bibliography for British, if not Turkish sources, embellish this useful study of improvised war on the colonial periphery.

Drexel Institute

TRUMBULL HIGGINS

MODERN YEMEN: 1918-1966. By *Manfred W. Wenner*. [The Johns Hopkins University Studies in Historical and Political Science, Series LXXXV (1967), Number 1.] (Baltimore: Johns Hopkins Press. 1967. Pp. 257. \$6.95.)

THE rapid shrinkage of the world in terms of time and distance in recent years has made the result inevitable that Yemen could not continue indefinitely in isolation. The problems faced by the Yemenis in their attempts to bring time-honored absolutism under the Imams into some more modern political semblance and steps taken by Egypt under Nasser to exploit these attempts in its own interest have provided warrant, if any were needed, for this first study in the English language of modern Yemen with Yemeni civil war still in progress.

The author has found it no mean task to provide the necessary background and to bring his account of a little-known country in some detail through the years of independence from the Ottoman Porte to the troubles of the recent past. To this end he has found it essential to range far afield in a number of languages for the requisite source materials; the bibliography resulting from this search is

one of the important features of the book. While the reader's attention may be diverted slightly by occasional imperfections in composition, the multitude of footnote citations attests the conscientiousness with which the sources have been combed in an effort to sift essentials from heterogeneous and sometimes conflicting accounts.

Wenner has done well indeed, on the whole, in bringing out the many kinds of obstacles that have impeded Yemeni nation building in an international environment charged with power politics. It seems not to have been his purpose to deal extensively with the international rivalry that has been manifested in Yemen's external relationships since the creation of the Yemen Arab Republic in 1962, yet the book surely would have gained somewhat in significance had more attention been given to these international relationships even to the point of including the degree of embarrassment experienced by the United States in having recognized the republican regime before its large measure of dependence on the United Arab Republic had become apparent. At all events, there can be no gainsaying the author's conclusion that "events . . . have amply demonstrated that the establishment of a 'modern' state in place of the traditional monarchy has not been the panacea for Yemen's problems its authors expected it to be. . . . Consequently, it seems unlikely that the almost insurmountable internal problems which characterize Yemen will be solved in the near future."

Center for Strategic Studies

HALFORD L. HOSKINS

A MANDATE FOR ARMENIA. By *James B. Gidney*. ([Kent, Ohio:] Kent State University Press. 1967. Pp. x, 270. \$7.50.)

THE story of a forgotten subject is well told in this first work of Mr. Gidney. The mandate of the post-World War I Armenian state, tendered to the United States and then summarily rejected by Congress, has a tangled and twisted history, but Gidney succeeds in reducing it to a readable narrative.

The wholesale massacre of the Armenians in Turkey early in the war had shocked the world's conscience, and Americans in particular were deeply moved by the tragedy. There was much talk of doing something for the surviving Armenians; this feeling of overflowing sympathy was expressed in newspapers, on pulpits, and in political speeches, bringing out our time-honored sympathy for the downtrodden and the underdog. The Armenians were, of course, elated by such expressions and seemed overjoyed in seeing a protector in the government of the United States. Not aware that these expressions did not constitute solemn pledges, they were misled. Even politicians here did not seem to realize that in our innocence and inexperience in world affairs we could not and perhaps would not help the Armenians in their distant homeland. President Wilson, however, seemed to have felt optimistic about it, and he acted accordingly. From Paris he sent two missions to the Near East (the King-Crane mission to southwestern Turkey and the Harbord military mission to Armenia) to study conditions. The reports of both missions cautioned the President; neither urged the acceptance of a mandate for Armenia alone, the King-Crane mission suggesting a mandate for Armenia, Anatolia, and Constantinople together. But the President persisted in his view. Finally, on April 28, 1920, the Allies offered the mandate of Armenia

to the United States. On May 24 the President asked Congress authority to accept it, but Congress decreed otherwise. The Senate Foreign Relations Committee voted (without even holding a hearing) eleven to four not to give the President the requested authority. And on June 1 the Senate (the House concurring) voted fifty-two to twenty-three (twenty-one abstaining) not to grant "the Executive the power to accept a mandate over Armenia." That proved the end of our near involvement in the Middle East, bringing in its train the rape of the dying Armenian state by the Turks and the Soviets and part of its Sovietization in December 1920.

The author has used effectively a large variety of archival material (except the important Westermann Diary, in Columbia University Library), in addition to nearly all important published works. Somehow the valuable House and Seymour volume *What Really Happened at Paris?* (1921), wherein Westermann has a chapter on Armenia, is neither consulted nor included in his "Bibliography."

Library of Congress

A. O. SARKISSIAN

MISSION IN PALESTINE, 1948-1952. By *Pablo de Azcárate*. (Washington, D. C.: Middle East Institute. 1966. Pp. viii, 211. \$6.75.)

READING the memoirs of a principal participant in the efforts to mediate the first Arab-Israeli war is a sobering experience. In the light of the hostilities of June 1967 Dr. Pablo de Azcárate's description of the 1948 clash between the Arab states and newborn Israel provides tragic evidence of the inability of those involved to learn from history and thus lay the basis for permanent peace in the Middle East.

Azcárate rightly ascribes the basic difficulty in Palestine to the failure of the British mandate to achieve a workable compromise between conflicting nationalist aspirations. He notes that the actual mandate administration was entrusted by the British government to the Colonial Office, "a department admirably suited . . . to ensure the domination of the white race over primitive peoples." But Arabs and Jews alike are peoples with old traditions and a high level of social organization; thus the mandate was designed wrong from the start.

Unfortunately the least valuable sections of the book deal with the author's personal record of the Palestine Conciliation Commission. Palestine as an issue in world affairs has yet to receive fully impartial analysis; Azcárate's particular syndrome stems from his commitment to the United Nations. He criticizes the United States for not providing the "indispensable" support to the commission that it needed to function effectively in its mediation effort. He castigates Britain for conniving at—even aiding—the creation of an embryo Jewish state under the mandate. The British in Palestine are taken to task for impeding efforts of the UN to implement the 1947 partition plan. The entire record, in fact, is marred by the sort of petulance regarding personalities, conditions of living, and insufficient respect for official status that a public official might preserve in his diary, but scarcely transfer wholesale to a book.

The story of Israel's evolution as a state, however badly presented, deserves every retelling if only in hopes that at some point understanding will bring posi-

tive action. The clash between Arab nationalism and Zionism has produced a problem that is difficult but not insoluble. As Azcárate says, time, good advice, friendly pressure, if necessary even threats and sanctions, will eventually make possible an honorable and effective compromise. In this respect his epilogue (written in 1965) is prophetic. The transfer of the commission from Jerusalem to New York removed a principal conduit for possible normalization of relations between the Arab states and Israel. The value of the UN as an instrument of mediation in Palestine was illustrated graphically by the results of withdrawal of UNEF troops from Gaza. Azcárate suggests positive steps toward compensation for Arab refugees from Israel as part of their total reintegration into the Arab countries.

Yet the most prophetic note is struck in the chapter "Looking Forward": "the more Israel orientates [*sic*] her policy towards a strengthening of her political and economic ties with the United States, the less likelihood there is of her being able to fulfil the role of instigator and guide in the great enterprise of the political, economic and social transformation of the Arab populations in the Middle East." Events since this record was set down merely reinforce the necessity for Israel to become a part of the Middle East, and not to remain a festering incrustation on its surface.

American University

WILLIAM SPENCER

POLITICAL PARTIES IN LEBANON: THE CHALLENGE OF A FRAGMENTED POLITICAL STRUCTURE. By *Michael W. Suleiman*. (Ithaca, N. Y.: Cornell University Press. 1967. Pp. xxiii, 326. \$7.50.)

LEBANESE politics, in the very nature of things, is probably the most complicated in the Arab world, perhaps because of the confessional structure of the state. Professor Suleiman, however, has done much to clarify the situation for the Western reader and student. For one thing, he begins with a historical background that sets political development in an appropriate perspective, without which it would be impossible to have any understanding, and he explains the present setting, with its regional differences, family, clan, and feudal ties, education, economy, the press, confessionalism, and the constitutional and political factors in Lebanon. He then proceeds to analyze the various political groupings, beginning with such transnational parties as the Communist party, the Syrian Nationalist party (PPS), the Baath and the Arab Nationalists' movement. Next come the religious and ethnic groups (including the Armenian), and finally the exclusively Lebanese parties, with stress on the An-Najada, the Progressive Socialist, the Phalange (Al-Kataeb), the Constitutional Union and National Bloc, and the National Liberals' party.

It is probably impossible for a foreigner, even when he has lived in a country, to sense the essence of its political structure, and even when party programs proclaim the principles on which the party takes its stand. This is especially true, one would think, in Lebanon, where parties appear to cluster about family, clan, and tribal, and religious leadership. But Suleiman examines both the programs and the leadership in highly understandable terms and presents the structure for what it is, with competition for political power and political socialization among

the functions of the contending parties. As the author also notes, the parties define political issues and attempt to "educate"—or mislead—their followers concerning their views. But he also observes that the parties tend to reinforce confessional and other particularistic attachments in this fragmented society, a process that is also reinforced both in the higher political offices and in recruitment for the Lebanese civil service.

Suleiman has done well, indeed, with his subject. Students of Lebanese and Middle Eastern politics will be indebted to him. Not only is his book well and clearly written; it is also replete with illustrative tables, and he closes with a selected bibliography of materials both in Arabic and Western European languages.

American University

HARRY N. HOWARD

Africa

TRADE UNIONISM IN AFRICA: A STUDY OF ITS GROWTH AND ORIENTATION. By *Jean Meynaud* and *Anisse Salah Bey*. Translated by *Angela Brench*. (London: Methuen and Company; distrib. by Barnes and Noble, New York. 1967. Pp. 242. \$11.50.)

IN most parts of Africa trade-unions now exist almost entirely on governmental sufferance. Feared by various colonial administrations, trade-unions have in recent years seen their powers and rights circumscribed by the new independent nations. Indeed, many regimes simply use the trade-unions as instruments of state policy.

The authors of this book, whose work has been adequately translated from French, devote their efforts to a description of the historical development of trade-unionism in Africa. Originally weak associations primarily interested in bettering the social and economic environments of their members, trade-unions subsequently involved themselves in political movements of nationalism. As a result of this involvement they grew powerful, in the process often subordinating their economic policies to political stratagems. Some trade-unionists even became, like Sekou Touré, politicians of consequence after transforming their labor movements into nationalist parties.

The unions benefited overwhelmingly from their affiliation with the victorious nationalists, but soon after the struggle for independence had succeeded, and their leaders had become members of the government, trade-unions atrophied. Having become political, they could hardly oppose with ease the very governments that they had so recently helped to acquire power. New legislation or police action hastened the process, and the trade-unions were characteristically instructed to play essentially secondary roles. As the authors of this book sensibly assert, African trade-unions now can usually do no more than to express the discontent of some workers; in most areas they are no longer capable of bargaining in industrial affairs. They also argue cogently that strong classical trade-unionism is an obstacle to economic development; this assertion is often made by African officials responsible for repressing strikes by the very unions that they once led.

Despite the general acceptability of its conclusions, this is an inferior book that adds little that is original to the study of trade-unionism in Africa. The authors obtained their data largely secondhand, and their examination of the problem is derivative and unimaginative. Despite a pretentious theoretical framework, the approach is decidedly descriptive. Their subject is important, but a definitive analysis must still be awaited.

Harvard University

ROBERT I. ROTBERG

IBN KHALDŪN IN EGYPT: HIS PUBLIC FUNCTIONS AND HIS HISTORICAL RESEARCH (1382-1406). A STUDY IN ISLAMIC HISTORIOGRAPHY. By *Walter J. Fischel*. (Berkeley and Los Angeles: University of California Press. 1967. Pp. ix, 217. \$5.00.)

EARLY in this book the reader gets an apprehensive feeling, similar to the one experienced upon tuning in on the middle of the Late Late Show, that he has seen it before. And indeed, in this case he has, for Professor Fischel here admittedly presents a summation of his earlier work on Ibn Khaldun, especially of his articles that have appeared in various *Festschriften* and his small book, *Ibn Khaldūn and Tamerlane*. Nothing very new or startling is presented in any of the three major divisions of this work: Part One, "Ibn Khaldūn's Life and Public Functions in Mamlūk Egypt"; Part Two, "Ibn Khaldūn's Historical Research in Egypt"; and Part Three, "Ibn Khaldūn and His 'Autobiography.'" The overall impression created by bringing this previously diffused material together in one place is flattering neither to Fischel nor to Ibn Khaldun.

Fischel is one of the main architects of the cult of personality that has grown up around Ibn Khaldun. Hard pressed to narrate a coherent and consistently favorable story in the face of a serious shortage of hard facts, Fischel is too often reduced to such weasel words as "must have been," "it seems that," and "most probab.y." The picture given of intellectual life in Cairo is woefully inadequate. To what, for example, are we to attribute Ibn Khaldun's obvious failure to attract any seriously committed disciples or students. Was it his unbridled egoism or the fact that he was an outsider coming into a system characterized by plurality of office and vicious struggles for control of pious foundations? The absence of literary style is the price paid for being too close to one's sources, and this book is no exception. More attention might profitably have been paid to the mechanics of scholarship. There are far too many errors in the citation of Turkish titles.

Of a more serious nature, however, is the image of Ibn Khaldun that emerges, or more precisely, does not emerge from this book. Admittedly, the *Muqqadimah* is a great work, and its author a great intellect, but he was not a great man. Fischel is much too partial to his subject. He gives Ibn Khaldun the benefit of every doubt, but even Fischel must admit, after a tortured attempt at seeking an alternate explanation, that Sultan Barquq dismissed Ibn Khaldun as chief Malikiite *cadi* in 1390 because he had been among the signers of the 1389 *futwa* that gave legality to the rebellion against that sultan who had been his benefactor.

It is rather obvious that Ibn Khaldun was not a man for all seasons. He was a sycophant, an opportunist, and a backbiter. What has been lacking in the vast literature on Ibn Khaldun listed by Fischel under the charming heading of "Ibn

Khaldūniana" is a psychologically oriented portrait of this justly famous figure. Only when such a study is available to flesh out the existing intellectual skeleton can we hope to discern a response when we ask the question, "Will the real Ibn Khaldun please stand up?"

Princeton University

NORMAN ITZKOWITZ

WEST AFRICAN KINGDOMS IN THE NINETEENTH CENTURY. Edited with an introduction by *Daryll Forde* and *P. M. Kaberry*. (New York: Oxford University Press for the International African Institute. 1967. Pp. xiv, 289. \$8.00.)

THE studies that make up this volume analyze the political, social, and economic institutions of ten precolonial West African societies: Benin and Oyo in Nigeria; Dahomey; Maradi in Niger; Kom in Cameroun; Mossi in Upper Volta; Gonja and Ashanti in Ghana; the Mende country in Sierra Leone; and Kayor in Senegal. The title of the book is slightly misleading since not all the political units described were kingdoms. Indeed, in the editors' introduction and in some of the studies words like kingdom, chiefdom, state, polity, and hegemony are employed more or less indiscriminately to signify what is being written about. But the purpose of the volume is clear. The contributors were asked to provide for each of their areas of interest an outline of historical developments prior to the nineteenth century, followed by a descriptive analysis with the main emphases on territorial structure, administrative and judicial procedures, methods of economic exploitation, social stratification, military organization, external relations (where significant), rituals of kingship and officeholding, and modes of political and social control. The result is a most interesting volume, which brings together in one place a wealth of new information derived from written sources, oral tradition, and observation in the field.

The majority of the studies have been written by professional anthropologists. The result, to a historian, is enlightening, but also in some degree and in some places confusing and even a little dubious. On the one hand, it is true, as one contributor says, that historians of precolonial West Africa have not always shown adequate understanding of social systems and have sometimes been too easily "mesmerized by dates and events." It is therefore salutary to see the past of African societies being laid open by specialists more familiar with the languages and customs of an area than is the average historian. On the other hand, the authors of these studies frequently jump from the past to the present tense and back again, and the reader is left wondering what justification there is for assuming that a particular procedure or ritual was the same in the nineteenth century as it is now (or was at the date of the writer's most recent field trip). The social science approach, too, is highly impersonal. With some exceptions it is institutions that are described here, not people, modes of behavior rather than the deeds of actual men and women. We are presented with the framework within which ten groups of nineteenth-century Africans are presumed to have operated. This is not history; it is, however, indispensable source material for the historian.

Taking the volume as a whole, one gains an impression of the enormous

complexity and subtlety of indigenous African political and social life before the colonial period. In itself, this is nothing new, but the facts have not been so clearly or authoritatively set out before. When more studies of the type represented here have been made, a definitive reconstruction of the West African precolonial past will begin to be possible.

Columbia University

GRAHAM IRWIN

GUGGISBERG. By R. E. *Wraith*. [West African History Series.] (New York: Oxford University Press. 1967. Pp. viii, 342. \$6.75.)

GORDON Guggisberg was not a standard colonial governor, and this is not a standard gubernatorial biography. It is more nearly an appreciation of the man than a study of his official acts and influence. Written before the recent opening of the Colonial Office records into the 1920's, it wisely avoids subjects that can only be treated fully when these records are available. As a result, it makes no claim to be definitive, nor to deal with the history of the Gold Coast during Guggisberg's governorship between 1919 and 1927. Guggisberg's personality and springs of action nevertheless come through clearly in a well-written and enjoyable book.

That a Canadian of Swiss and Jewish ancestry should ever have become a colonial governor is unusual enough. That this should have been done by way of an army career in the Royal Engineers and assignment to West Africa as a surveyor was almost unheard of, even though the achievement apparently owed more to the influence of Guggisberg's wife than it did to recognition of his abilities in high places. He succeeded, however, in making himself the best-remembered of all those who ruled the Gold Coast. As Professor Wraith points out, this was partly owing to Guggisberg's gift for public relations, but his achievements were also more personal than is often the case with governors caught between the Colonial Office above and the secretariat below. This was especially so with his creation of an infrastructure for later economic development and of his work in education.

The Gold Coast that was to be developed and educated, however, is seen here only in the distant background. Wraith's point of view is from the heights of the governor's residence, and somewhat from that of an English expatriate, however sympathetic with modern Ghana. His strength is in another direction, especially in the way he captures the attitude of mind that might be called the mentality of colonialism at its best. Guggisberg was intensely conservative, but his conservatism was joined to a paternalistic drive for "progress." His type of paternalism is now out of fashion, and progress in modern Africa is defined differently. Perhaps this book's chief service will be to recall what colonial rulers thought they were doing only a few decades ago.

University of Wisconsin

PHILIP D. CURTIN

THE ZAMBESIAN PAST: STUDIES IN CENTRAL AFRICAN HISTORY.
Edited by *Eric Stokes* and *Richard Brown*. (New York: Humanities Press.
1966. Pp. xxxv, 427. \$8.50.)

WITHIN the past few years it has become fashionable in the writing of African

history to produce collections of individual essays by numerous contributors. This is not surprising. Given the enormous difficulties confronting the historian of Africa and the skills required to discover the African past, a collection of essays by specialists, working in depth within the limits of their own competence, makes not only good sense but good reading. The increasing use of the conference as a method to focus attention on particular regions or problems has invested the African collaborative work with a legitimacy it has virtually lost in European and American historiography.

The following sixteen essays were prepared for the Seventeenth Conference of the Rhodes-Livingstone Institute held in May 1963 and their publication sponsored by the institute's successor, the Institute for Social Research, University of Zambia. There are always obvious risks in publishing a disparate congregation of essays. It is fortunate that they are not present in this excellent volume which does much to clarify the Zambesian past. The editors have not only produced a volume in which the highest quality of scholarship and expression prevails throughout but to which they themselves have made perceptive contributions. Richard Brown's "Aspects of the Scramble for Matabeleland" demonstrates the importance of the nature of the Matabele state in the European occupation of Rhodesia. Eric Stokes has prepared two essays, "Barotseland; The Survival of an African State" and "Malawi Political Systems and the Introduction of Colonial Rule 1891-96." In the first Stokes reiterates for Barotseland the theme that Brown applied to the Matabele: the political and social organization of African society conditions, if it does not determine, the nature of colonial rule that follows. Only when Stokes carries this theme superficially beyond the death of Lewanika does his essay acquire a peculiar and unnecessary afterthought. His second contribution establishes for Malawi what historians are discovering in other areas of British Africa. The implementation of indirect rule through the indigenous authorities was possible only when the incomers were militarily strong. When they were weak, as in Nyasaland, the British officials had to break the power of the indigenous rulers in order to assert their control.

The two essays by Terence Ranger, "The Role of Ndebele and Shona Religious Authorities in the Rebellions of 1896 and 1897" and "Traditional Authorities and the Rise of Modern Politics in Southern Rhodesia, 1898-1930," are two excellent pieces of scholarship, demonstrating the importance of the leadership that the traditional religious and political authorities provided during the transition from African to European control. The role and influence of the Mwari cult and spirit mediums have been virtually overlooked, probably because the Rhodesian officials themselves never understood their significance. This was not the case with Lobengula's successors. The Rhodesian authorities knew only too well the influence of the House of Lobengula and worked effectively to see that it did not become a rallying point for Matabele nationalism.

Finally, P. R. Warhurst's "The Scramble and African Politics in Gazaland," Ian Cunnison's "Kazembe and the Arabs to 1870," and J. R. Rennie's "The Ngoni States and European Intrusion" are all concerned with more well-known subjects to which each has added significantly, if not in terms of information, at least with fresh insights. The two shorter essays, the first by Mutumba Mainga, "The Origin of the Lozi: Some Oral Traditions," and the second by L. S.

Muuka, "The Colonization of Barotseland in the 17th Century," are useful, if somewhat undigested contributions. The least satisfying pieces are, perhaps, G. K. Garbett's "Religious Aspects of Political Succession among the Valley Korekore" and Ann Tweedie's "Toward a History of the Bemba from Oral Tradition," where the authors have failed to separate the anthropological chaff from the historical wheat. This by no means compromises an essential volume in the historiography of Central Africa, and one can hope that it will convince foundations to support more conferences by historians of Africa and hesitant publishers to produce the results.

University of California, Santa Barbara

ROBERT O. COLLINS

Asia and the East

L'ASIE ORIENTALE AUX XIX^e ET XX^e SIÈCLES: CHINE—JAPON—INDE—SUD-EST ASIATIQUE. By *Jean Chesneaux*. [Nouvelle Clio: L'histoire et ses problèmes, Number 45.] (Paris: Presses Universitaires de France. 1966. Pp. 371. 20 fr.)

THIS compact volume makes an extensive review of the major historical problems that have confronted modern Asia, at least that area which French geographers call "Asia of the monsoons." Writing for the introductory student, Jean Chesneaux has divided his presentation into three parts. The first, containing an up-to-date annotated bibliography of Western primary and secondary sources, is particularly useful for information on French archives. The second part consists of nine chapters, giving a narrative account of Asian history that clearly reveals the author's emphasis upon Western imperialism and his opinion of its profound influence on the course of Asian history. His last section is an interpretive essay in which the author attempts to establish related patterns of development between the different areas of East, Southeast, and South Asia.

While Chesneaux recognizes the importance of internal developments and traditional values, he regards the Western thrust into Asia as the most important in affecting the political, economic, and social changes that have taken place there in modern times. It was not until the mid-1950's that Asia passed from a "euro-péo-centrique" perspective to one of "asio-centrique." Most important in this development were the events leading to World War II (1937-1941) that ultimately separated the colonial period from that of emerging nationalism, a concept that preoccupies the author.

Chesneaux raises many fundamental questions that he believes remain unanswered, such as the extent to which Marxism has been accepted in Asia, its lasting influence there, and the need to explain the inability of some Communist parties in Asia to lead nationalist movements (that is, India and Indonesia). He also presents diverging historical views and sometimes takes issue with them. He does not fully agree, for example, with Conrad Brandt, Benjamin Schwartz, and John K. Fairbank on the originality of "Maoism," but regards the success of Communism in China and Vietnam as "without doubt one of co-ordination between peasants and workers."

Although Chesneaux makes a creditable presentation of the "expansion of

Europe" theory, his effort to portray all of East, Southeast, and South Asia in a uniform historical perspective still gives too little credit to the significantly different responses to the West in this area and the genuinely limited effect of Western influences on most of the people there, at least until 1949. While there is a certain uniformity in Western "imperialism" and opposition to it, other indigenous factors play an infinitely important role in the success or failure of modernization in Asia.

Purdue University

LEONARD GORDON

OU-YANG HSIU: AN ELEVENTH-CENTURY NEO-CONFUCIANIST.

By James T. C. Liu. [Stanford Studies in the Civilizations of Eastern Asia.] (Stanford, Calif.: Stanford University Press. 1967. Pp. viii, 227. \$6.50.)

THIS study, the first in a Western language on its subject, consists of two parts: six short chapters on the Sung historical setting and on Ou-yang's biography; six chapters devoted topically to his contributions in the many fields with which he was concerned—classics, history, political theory, literature, and thought. Professor Liu's survey, good as it is, is only an introduction to the life and work of this fascinating virtuoso. We must hope that Liu or some other scholar will fill out the picture from the abundance of official and private sources that are available. I particularly miss, in this volume, evocations of landscape and milieu. What kind of a place, for example, was Yingchow where Ou-yang finally settled, and what social circles did he find in the provincial posts to which he was sometimes relegated? I am thinking of the sort of vivid detail that Lin Yu-tang provided in his life of Su Tung-p'o or Arthur Waley in his study of Yuan Mei.

Of the topical chapters, that on "Rationalism and Religion" is perhaps the best. In it Liu depicts a Neo-Confucian of the early stages of that movement—a devoted protagonist of the "ancient style," a moral zealot, a staunch rationalist yet a man of refreshing skepticism who was fully aware of the limitations of his creed. It is natural that the master synthesizer of the tradition, Chu Hsi (1130–1200), should have dismissed Ou-yang as a lightweight, too frivolous a man for the solemn puritanical zealot that became, in time, the ideal of the "School of the True Way." For us this makes him all the more appealing. He was a major poet, one of the innovators in the *tz'u* form, and by no means all his themes found favor with the stern moralists who followed him as certainly poorer poets if, in their own eyes, more proper defenders of the "True Way." Liu seems to quote with approval Chang Hui-yen's view that Ou-yang's love poetry was political allegory. I find this quite unpersuasive, especially since the analogy is to the moralistic interpretation of the *Book of Songs*, one of the most absurd exercises in scholasticism in the Chinese tradition equaled in the West by the Christian effort to moralize the Song of Songs.

I find Liu's estimate of Ou-yang as a historian decidedly overgenerous. He properly reviews Ou-yang's interest in the raw materials of history: his archaeological and bibliographical works and his efforts to order the preservation of contemporary records. Ou-yang's reputation as a historian rests on his authorship of the *New T'ang History* and the *New History of the Five Dynasties*. Of the latter Liu says: "Ou-yang's new history was chiefly interpretive. He paid little

attention to events, many of which he dismissed as inconsequential occurrences of a chaotic period or undesirable results of Buddhist influence. . . ." Yet the omission of Buddhist activities in both his histories—covering an age when Buddhism was the dominant faith of all classes—can only be judged as a gross distortion of the record. Here is what a monk-historian of the thirteenth century said of him: "When Ou-yang wrote his histories of the Five Dynasties and of the T'ang, when he came upon Buddhist matters, he expunged them. Now the T'ang History is the standard history of the T'ang house and not the personal expression of Ou-yang. . . . Is it right that he should take the things he disliked personally and expunge them from the record? Such a person is unfitted for the responsibility of writing history. . . ." I would agree with this judgment, and I am unimpressed by the statement that these histories "did have enough merit of their own to be included among the Standard Histories." The people who granted them this exalted status were moved less by objective standards of quality than by literary preoccupations and by the overwhelming moral zealotism of the Neo-Confucian movement.

Despite all these reservations I found Liu's pioneering study engrossing and stimulating. It breaks free of many stereotypes, if not all of them, and introduces to the Western reader one of the truly great men of the Chinese tradition.

Yale University

ARTHUR F. WRIGHT

PRELUDE TO HONGKONG. By *Austin Coates*. (New York: Hillary House, 1966. Pp. xi, 232. \$8.00.)

THIS book, by the author of *Invitation to an Eastern Feast* (1953), *Personal and Oriental* (1957), *Basutoland* (1966), and other apparently relatively popular works, covers the relations between England and China from 1637 to 1842; it is not an easy work to evaluate fairly. If one wants a brief account of early Anglo-Chinese relations, with much about Macao thrown in, that is precisely accurate on all factual matters and never goes beyond what can be supported from documents, then I cannot recommend this book. It has numerous minor factual errors, and there could be considerable disagreement over some of its larger themes and interpretations. If, however, the reader wants a rather concise account of early relations between England and China that touches on the major points, is quite readable, and presents a basically sound view of the relationships, with many insights that the scholarly accurate factual narrative quite often omits, then he will find this book of interest and value.

One is in fact somewhat puzzled about various things in the book. It has a short bibliography of basic sources and interpretive works, although a large number of equally important studies and sources are not mentioned, especially Arthur Waley's *The Opium War through Chinese Eyes* (1959) and Hsin-pao Chang's *Commissioner Lin and the Opium War* (1964), in view of its constant effort to take us behind the scenes and show us what really happened or how the Chinese really thought. There are practically no footnotes to support many novel and challenging statements and interpretations, yet it is clearly evident that the author has read much material not mentioned in his notes or bibliography.

The book is dedicated to J. M. Braga, the Portuguese student of Macao resident in Hong Kong, to whom he indicates his indebtedness for various things. In fact he states that much of the material about the internal conditions of Macao are derived from oral tradition (which at this date must be the information in the minds of local scholars like Braga), and one suspects that this applies to developments in other parts of the region as well.

One could cite several pages of specific inaccuracies and questionable but undocumented statements or interpretations, but despite all these the work presents a generally sound and balanced account of these early years of Anglo-Chinese relations.

University of Arizona

EARL H. PRITCHARD

THRONE AND MANDARINS: CHINA'S SEARCH FOR A POLICY DURING THE SINO-FRENCH CONTROVERSY, 1880-1885. By *Lloyd E. Eastman*. [Harvard Historical Studies, Volume LXXIX.] (Cambridge, Mass.: Harvard University Press. 1967. Pp. xiii, 254. \$6.00.)

THE conflict between France and China in the early 1880's is one of the more obscure episodes in modern international relations. The contestants were moved by *amour-propre*: the chief purpose for France was to manifest French grandeur by somehow, someway acquiring an empire in the East; for China, the maintenance of China's traditional overlordship as embodied in the tributary system. The atmosphere in which this encounter took place was rendered almost impenetrably murky by adventurism, miscalculations, odd military alarms and excursions, disingenuousness, confusion of voices, and domestic political controversy on both sides. By using this controversy as a test case for the analysis of the decision-making process in the latter days of the Chinese Confucian state, the author of this very competent study has illuminated both the workings of Chinese politics and the Sino-French confrontation itself. The three major elements involved in the jockeying for power in late nineteenth-century China were the throne (that is, at this time the Empress Dowager), where authority resided, but where the retention of actual power necessitated adroit political maneuvering; the great provincial officials, made strong in the process of suppressing the Taiping rebellion, of whom Li Hung-chang was not only the best representative but also the one most deeply involved in this crisis; and *ch'ing-i* (pure discussion), the opinion of the mandarinates, expressed in conventional Confucian terms by a group of younger officeholders. Although the author is scrupulous in not exploiting the resemblances and relationships between this nineteenth-century Vietnam problem and its present-day successor, it is impossible not to note the hawkish bellicosity of *ch'ing-i*, the dove-like reluctance of Li Hung-chang, and the dilemma of the throne, which the Empress Dowager finally resolved by ceasing to receive *ch'ing-i* and agreeing to peace.

The book is written in clear and crisp style, is based on a substantial range of Chinese, French, and English sources, and has a glossary of Chinese names and terms and an index. Altogether it is a worthy addition to the series of "Harvard Historical Studies."

Mount Holyoke College

MERIBETH E. CAMERON

THE CHINESE COMMUNIST ARMY IN ACTION: THE KOREAN WAR AND ITS AFTERMATH. By *Alexander L. George*. (New York: Columbia University Press. 1967. Pp. xii, 255. \$6.95.)

THE ROLE OF THE CHINESE ARMY. By *John Gittings*. [Issued under the auspices of the Royal Institute of International Affairs.] (New York: Oxford University Press. 1967. Pp. xix, 331. \$8.50.)

MR. George has taken advantage of about three hundred interviews with Chinese Communist prisoners captured in Korea in 1951 to focus on the problem of why that underarmed, underequipped army fought so effectively. His answers draw from the make-up of the military institution, specifically, for example, how the political and social practices of the revolutionary cadre motivated the soldiers. Among the techniques employed, self-critical group therapy apparently worked well, at least until the superior firepower of the United Nations forces destroyed the illusion of invincibility. George has also analyzed the reorganizations the People's Liberation Army (PLA) have undergone since the Korean War.

Writing a history of the PLA from 1946 to 1965, Mr. Gittings has presented a detailed picture not only of the military but of Chinese society itself. He has discussed the role of the army in the state, its relationships with other political and social segments, and its own internal composition and methods of operation. In doing so, he has described the curious circle it made in organization, doctrine, and leadership. A revolutionary army in the late 1940's, it became a professional force, then turned back to the earlier tradition, particularly by reinstituting the apparatus of dual political and military command and control.

Brilliant work by mature scholars, both volumes are especially welcome today. They illuminate the recent disturbances that have erupted in China and send ominous tremors throughout the world.

Alexandria, Virginia

MARTIN BLUMENSON

SOCIAL LIFE IN ANCIENT INDIA (IN THE BACKGROUND OF THE *YAJÑAVALKYA-SMṚITI*). By *Sudhakar Chattopadhyaya*. (Calcutta: Academic Publishers. 1965. Pp. ix, 178. Rs. 12.50.)

THE ancient Indian law codes and commentaries on them are a veritable treasure house of information on ideas and conditions of society in India from about 200 B.C. to the thirteenth century A.D. and beyond. Much of the information contained in them has already been collated and interpreted in a number of works published so far. Some of the individual smritis can still bear deeper analysis and further discussion, and Professor Chattopadhyaya has attempted to do it for the Yajnyavalkya-smṛiti.

The work is in three sections comprising a total of twelve chapters. The range of subjects discussed is extensive, covering as it does the caste system, education, marriage, household life, occupations, position of women, slavery, public women, untouchability and slavery, and the institution of sannyasi. The author accepts "early days of the Gupta rule" as the probable date for the Yajnyavalkya work and is mainly concerned with the general subject of parallels and differences between Manu and Yajnyavalkya on the one hand and Kauṭalya and Yajnyaval-

kya on the other. He has succeeded in accomplishing his aim to a substantial extent. But, it seems to me, that if that was all that was involved it could as well have been done within the compass of a substantial paper in a journal, and it was not at all necessary to write a 178-page book for that purpose. Indeed the rather ambitious scope of the topics discussed has seriously affected the nature of the work, for many of them are much too complex to make their discussion meaningful through a cursory analysis. Concepts such as caste and untouchability need rigorous definition which is sadly lacking in this work. It is difficult to understand to whom this book is addressed; the general reader will find it baffling, and the specialist will be disappointed. The author's erudition is obvious; what is not so clear is his skill in analysis and a desire to go beyond the bare "facts" gleaned from a reading of the smṛiti on which he bases his work. It is a convenient summary of work done so far on the general subject, but seems to add little of significance to what we already know.

Wake Forest University

B. G. GOKHALE

THE INDIAN MUSLIMS. By *M. Mujeeb*. (Montreal: McGill University Press. 1967. Pp. 590. \$12.75.)

THIS well-documented and well-written study, perhaps the most erudite effort of its kind published in recent years, is on one level a configurational analysis of Indian Islam and on another an interpretive history of India from the Muslim Turkish intrusion in the twelfth century to the present (1960). In painstaking fashion, Professor Mujeeb lays a solid foundation of political, religious, aesthetic, and social aspects of the sultanate period and then demonstrates how and why these components persisted or were modified by historical forces. Mujeeb's attempt to see the total culture pattern of South Asian Muslim civilization in historical perspective is clearly where he has made his strongest contribution.

Many specialists on South Asia, on the other hand, will probably look upon such an approach as overly ambitious and fruitless. The work of an increasing number of area specialists seems to indicate that they believe intensive regional monographs are potentially more useful than the sweeping monolithic studies of the subcontinent. From their viewpoint, projected books on Indian Muslims, or on any other religious, ethnic, or cultural grouping, should be set aside for the more urgent task of gathering data on Punjabi or Bengali Muslims, and so forth. They contend that long-debated questions such as whether there was or was not an Indo-Muslim syncretic civilization in South Asia may best be answered by confining one's research to the accommodation of Islam within a single well-defined regional culture at a given time in history.

Notwithstanding the convincing argument of the regionalists, Mujeeb has still given us a valuable addition to historical writings on Indian Islam. Though monolithic in his desire to characterize the style of life of "Indian" Muslims, he focuses on regional developments from recent sources and acknowledges the importance of regional differences. Specialists of South Asia will appreciate his determination to resist reading the present-day political myths of Hindu-Muslim animosity into the past. Equally important, Mujeeb wisely avoids broad general-

izations on the nature of Islam and any serious attempt to synthesize the culture fragments he so laboriously depicts. Nor are his observations on the popular appeal of Sufism, Akbar's unifying policies, the eighteenth-century decline of the Moguls, and the rise of Muslim nationalism daringly original. Indeed, it is precisely this disciplined restraint that is most refreshing about Mujeeb's style.

University of Minnesota

DAVID KOPF

THE ILLUSION OF PERMANENCE: BRITISH IMPERIALISM IN INDIA.

By *Francis G. Hutchins*. (Princeton, N. J.: Princeton University Press. 1967. Pp. xv, 217. \$6.50.)

THIS is a historical essay in the English (as distinct from the German) tradition. The author is concerned with the origin, development, and influence of the notion that England in the interest of justice should continue to rule India permanently. In pursuing this question he follows the course of evangelical and utilitarian doctrine, the Victorian code of conduct, concepts of Indian character, the structure of sahib society, the dissolution of company rule, and "orientalism," the new imperialism, and nationalism. The most original and incisive chapter, "Technology, Force, Democracy," is based on the Stephen Papers. All of these elements are shown to have reinforced the idea of a permanent British raj.

Hutchins writes in an elegant and unobtrusive style; he is, in fact, a stylist of no mean talent. And he makes particularly good use of literary evidence. But his method is not without flaws. In some instances, excessive reliance on secondary material has led him astray. William Hodson is cited as an exemplar of the Public-School ethos; Hodson was unquestionably an inspired guerrilla fighter, but he was poor at team games, had an aversion to hard work, and many of his Indian contemporaries considered him dishonest. Another instance is the pejorative newspaper description of Imam-ud-din cited secondhand from Edwardes' and Merivale's *Henry Lawrence*. This item reflects a specific political situation, not a general opinion of Indians. Imam-ud-din was the chief rival in Kashmir of the British-supported Dogra Rajput, Gulab Singh. Again, familiarity with the wills of some early Englishmen in India would not have permitted the author to think that children of mixed parentage were poorly provided for in those days.

One wonders why Hutchins did not exploit the papers of John and Henry Lawrence and juvenile fiction such as the novels of G. A. Henty, to say nothing of a host of neglected memoirs that might have furnished fresh evidence. Further investigation would have enabled him to flesh out his argument to advantage. As it is, he has produced a work that is far from negligible, yet one less substantial than Bearce's *British Attitudes towards India* (1961), to which it bears a family resemblance. Still, to tackle a broad theme of this sort requires much courage, and Hutchins has carried it off successfully. He has sorted out many threads and arranged them in delightful patterns.

Los Angeles Valley College

MARK NAIDIS

BRITISH POLICY IN INDIA, 1858-1905. By S. Gopal. [Cambridge South Asian Studies.] (New York: Cambridge University Press. 1965. Pp. xii, 422. \$13.50.)

For this study Professor Gopal has mined the vast bulk of British archival materials from the viceroynalties of Canning through Curzon, seeking "to examine the ideas and aspirations of British parties and statesmen, their ways and methods of implementing them and the consequences, both anticipated and unintended, of these efforts." Since most of the viceroynalties encompassed within this first half century of direct crown rule have as yet been unexplored by monographic research, the task of attempting to treat these fifty years as a cohesive "period" is, even for so accomplished a historian as Gopal, Herculean. The basic weakness of this work thus appears primarily to be a product of excessively ambitious conceptual scope, for instead of clarifying the "policy" that directed British Indian administration throughout so unwieldy a "period," this book serves to prove how impossible such a task of analysis remains. The product reads rather like an artificially united series of fragments, each of which could easily have been expanded into a separate, more meaningfully luminous, volume.

The work is divided into five chapters: "The Aftermath of Revolt, 1858-69"; "The Conservative Adventure, 1869-80"; "The Liberal Experiment—Ripon and Dufferin"; "Return to Caution, 1888-98"; and "Curzon: The Apogee of Administration." The "Conclusion" chapter is a brief but ineffectual attempt to assert the validity of the conceptual premise. Though the basic format is chronological, there are topical subdivisions within each chapter, at least to the extent of separating external from internal affairs. In the first chapter the viceroynalties of Elgin and John Lawrence are added (it would almost appear as an afterthought, since only some five pages are devoted to both) to a lengthy and diffuse consideration of Canning's viceroynalty. Canning's labors are summed up by Gopal in terms of "pettifogging and clumsiness," while Elgin, we are told, "died too soon" and "Lawrence came too late." The half decade of Lawrence's rule is dismissed as one that "saw little impact on the Indian scene." Despite the interlude of Gladstone's Liberal government, the twelve years from 1869 to 1880 are seen by Gopal as dominated by Disraeli conservatism; the Tory penchant for imperial expansion and preoccupation with the ideal of administrative "efficiency" are stressed as the polestars of this period's policy. The pendulum of British Indian rule then swings to its "Liberal Experiment" which, though undermined by Dufferin's ineptitude, was at least launched in modified form by Ripon. These second and third chapters are the best of the book. The next decade is described as "a period of marking time," while Curzon's era is characterized as "the apogee of British Indian administration" in which "efficiency" again became the primary goal of government. Gopal concludes with Curzon's resignation since, as he quite accurately puts it, "India after 1905 had new interests and objectives and compelled new lines of British policy."

University of California, Los Angeles

STANLEY WOLPERT

MORLEY AND INDIA, 1906-1910. By *Stanley A. Wolpert*. (Berkeley and Los Angeles: University of California Press. 1967. Pp. x, 299. \$6.95.)

PROFESSOR Wolpert has produced a lucid, provocative, and significant volume on John Morley's career as Secretary of State for India. It is eminently readable, though it can be argued that its topical arrangement detracts somewhat from its impact. It is not, however, an easy book to evaluate, for it has certain interesting limitations. Some of these bear upon our understanding of British rule in India.

Wolpert asserts at the outset his preference for an approach that is biographical and humanistic. He stresses the role of Morley and his colleagues in positions of authority instead of emphasizing structural and institutional factors. Thus he dwells upon relations between Morley and the governor-general, Lord Minto, when, in fact, the richer illumination might have come from a more careful study of the system of relationships between the office of the Secretary of State and the governing ICS bureaucracy in India. Wolpert's emphasis is upon the ideas, personalities, and whims of the individuals he names.

His initial chapter is a biographical sketch and intellectual history of Morley; it is not wholly adequate because it fails to come to grips with the complexities and limitations of the man and of Edwardian Liberalism. It is interesting that Wolpert does not notice that Morley published a sympathetic biography of Cromwell (1900), which, if taken into account, might have helped explicate some features of Morley's "liberalism."

Wolpert's assumption is that Morley was a peerless embodiment of all that was noble in the Liberal creed. Hence, Morley at the India Office could do no less than push for major reforms in India. To be sure, there were some bad men lurking in the underbrush who tried to thwart him, but on the whole Morley accomplished wonders against great odds. To make his case, Wolpert assembles all available evidence and at times indulges in that brand of history which tells us how bad things might have been if Morley had not been there to save the day. It is in good part an acceptable version, but at times it leads to interpretations that are, at best, strained.

In Chapter v, for example, Wolpert confronts the way Morley handled internal security and repression of unrest in India. Patently a leading Liberal could not lend himself to Draconian measures. Yet, of course, Morley sanctioned many such measures. Indeed, it can be shown that there was as much repressive legislation in India during Morley's regime as in any comparable period of time. Wolpert's treatment of the paradox is rather weak, relying too much upon assumptions about motives of personal pride, while giving the reader many trees but small view of the forest.

But the crux of his thesis lies, as it must, in the sixth chapter. Here Wolpert examines the Morley-Minto reforms (Government of India Act of 1909). The reforms were, in his view, a fundamental step in the development of parliamentary institutions in India. To reach this conclusion, it seems to me, Wolpert has overstated Morley's aims, used inference more than is appropriate, and understated the ways in which the crucial enabling regulations cribbed and whittled down the spirit of the reforms. His view seems to overlook the fact that the

reformed councils were constructed and controlled in a manner basically different, in several respects, from English parliamentary bodies.

By contrast, the chapters on "The Army and Foreign Affairs" and on "Laissez Faire versus Svadeshi" strike me as excellent. Here the author's partisan zeal for Morley, seen as the pre-eminent Liberal, does not have to stand in the way of a balanced and insightful interpretation. The situation was such that Morley could stand by his doctrines in military matters, while in economic affairs Morley's liberal opinions were obviously irrelevant to India's situation.

On the whole, if Morley had been studied more through the perspective of the role of his office and its interactions with the decision-making structures in India, the limitations inherent upon a Secretary of State could have played their appropriate explanatory function in Wolpert's account. The reader could have better seen how things happened and why they happened as they did when they did.

Duke University

ROBERT I. CRANE

SAM HIGGINBOTTOM OF ALLAHABAD: PIONEER OF POINT FOUR TO INDIA. By *Gary R. Hess*. (Charlottesville: University Press of Virginia. 1967. Pp. ix, 177. \$5.50.)

As the author tells us in the preface, this study began as a biography of Sam Higginbottom, but took shape also as a history of the Allahabad Agricultural Institute and a survey of the development of Christian agricultural missions in India in the early twentieth century. It remains, however, essentially a portrait of a dedicated Presbyterian missionary who understood his mission in terms of practical economic uplift and whose remarkable fund-raising talents helped his vision to materialize. Higginbottom's talents and drive suited his era perfectly. Had he arrived a generation earlier, the intensely evangelical Christian missions and the conservative Indian government would have put a damper on his enterprise; in the more recent period governmental and foundation programs have superseded large-scale missionary activity.

Hess belongs to the "warts-and-all" school of biographers. Although he does not slight Higginbottom's considerable personal achievement, he also pictures a man whose authoritarian personality made it difficult for him to get along with subordinates, who suffered a tragic estrangement from his children, and whose cultural outlook was decidedly "middlebrow." Hess's style is unembellished, clear, and factual. Some may think that the significance of this investigation could have been heightened by an increased emphasis on the state of Indian agricultural education at the time Higginbottom began and afterward, further elucidation of the precise economic problems faced by the Indian ryot, and more consideration of the extent to which the Allahabad Institute was able to grapple with these problems. The chapter on the "Years of Promise, 1920-1931," contains tables relating to the enrollment and expenditure of agricultural colleges in India. More such data would have been welcome. As it is, the reader is constantly reminded of problems of funding and minor personalities at an institution whose

relation to Indian society is imperfectly suggested. Higginbottom's notion of village development, for example, was undoubtedly more realistic than Gandhi's.

The main value of Hess's study is his detailed coverage of Higginbottom's career, but perhaps a more useful book could have been written with the Christian agricultural missionary movement in India as the main focus including Higginbottom's contribution as an important example. In any case, the present work is a dissertation of merit that deserves a place on the shelves of university libraries. And the University Press of Virginia is to be congratulated on producing it as an unusually handsome example of the bookmaker's art.

Los Angeles Valley College

MARK NAIDIS

THE INDUS RIVERS: A STUDY OF THE EFFECTS OF PARTITION.

By *Aloys Arthur Michel*. (New Haven, Conn.: Yale University Press. 1967. Pp. xxv, 595. \$12.50.)

THE scope of this book is broader than the title suggests. Almost one-quarter of the pages are devoted to descriptive geography of the rivers and plains in the Indus system and to an account of the programs and problems of canal building in the prepartition era, when the whole region was unified under British hegemony. There is lengthy analysis of the conflicting territorial claims of the Muslim League, Indian National Congress, and militant Sikh parties at the time of the boundary award in the Punjab during 1947. Thus, after exhaustive treatment of the background of partition of the Indus River, not all of which is essential to the reader's understanding of its effects, the author eventually reaches the main topic on page 195. The balance of the book deals with the water allocation crisis of 1948 and the roles played by engineers, lawyers, and statesmen in preparing plans, securing the agreement of India and Pakistan for division of the water resources, and carrying out the first stages of the construction of new canals and dams. The vast scale and extreme complexities of the irrigation, siltation, waterlogging, and soil salinity are described and documented. Economic aspects of current and projected water resource development receive less attention.

Professor Michel succeeds well in his purpose of providing information on all political and physical aspects of development under the Indus Waters Treaty. He does not hesitate to express his evaluation of the comparative advantages of the several schemes undertaken up to 1966 or projected thereafter. His appraisal merits attention because of his intimate knowledge of the region, particularly West Pakistan, and his previous experience in studying water resource problems of a neighboring country (*The Kabul, Kunduz and Helmand Valleys and the National Economy of Afghanistan* [1959]).

The book is a valuable source of facts and insights for historians, political scientists, and geographers concerned with South Asia. In my opinion, the work would have gained in utility by condensation, and the maps, while adequate for purposes of location, could have been much more effective in representation of terrain and hydrographic relationships.

Rutgers University

JOHN E. BRUSH

EXPERIMENT WITH FREEDOM: INDIA AND PAKISTAN, 1947. By *Hugh Tinker*. [Chatham House Essays, Number 16. Issued under the auspices of the Royal Institute of International Affairs.] (New York: Oxford University Press. 1967. Pp. x, 165. \$2.40.)

WHAT events led to the achievement of independence by India, and, more particularly, why was Pakistan created? These are the principle questions in this paperback by Professor Tinker. Both V. P. Menon's *The Transfer of Power in India* and Alan Campbell-Johnson's *Mission with Mountbatten* examine the same question with perhaps greater thoroughness and authority than does Tinker. But it is clear that the author wishes to complement rather than compete with such similar works, and thus he refers to this work as an essay.

It is a stimulating book more for the questions it raises than for those it answers. It almost suggests that the answers to the questions raised above, especially the creation of Pakistan, are to be found in the temperamental moods of the men who were negotiating. Lord Wavell, Gandhi, Jinnah, and Nehru were, we get the impression, a moody lot, each with his own peculiar quirks. Insufficient deference or inappropriate words or gestures would set off an entire historical sequence that might just as easily have been otherwise if the negotiators had been in better moods. Thus, who was to blame for the creation of Pakistan? (It is usually assumed that the division of the subcontinent was "a bad thing.") All of them, according to Tinker, were culpable in one way or another. What about the theory that the British did it as part of their usual policy of divide and rule? Tinker convincingly points out (opposed to the views of Mosley and Edwardes) that there is more myth than reality in that charge. Lord Mountbatten, indeed, in this essay is portrayed as an amiable fellow who did a difficult job well.

As an essay, the work has considerable merit. One feels, nevertheless, that even with the author's caveat that he is not going to divulge his sources, that far too many quotations, some lengthy, are given without references, and that the few footnotes in the book are sparse. The simplification of issues becomes almost caricature as when "The British 'saw Gandhi' as a kind of Mephistopheles," Nehru as a confirmed revolutionary, and Jayaprakash as "the guerrilla captain." "When Mountbatten met Jinnah, he recognized that this was the man who held the key to the whole Indian problem." "Nehru was a dreamer half awake." "The viceroy, as always, had grasped the fundamentals of the problem with sagacity and vision. . . ." An index, map, and bibliography would have been welcome.

University of Windsor

JOHN W. SPELLMAN

THE EMERGENCE OF PAKISTAN. By *Chaudhri Muhammad Ali*. (New York: Columbia University Press. 1967. Pp. ix, 418. \$11.00.)

At long last an account of the partition of India has appeared, told by one who participated in it as an official on the Pakistan side. Chaudhri Muhammad Ali was one of two members of the Steering Committee responsible to the Partition Council during the months preceding partition. With the establishment of

independence he became Secretary-General of Pakistan and a member of the cabinet. Later he was Finance Minister, and in 1955 he became Prime Minister. Under him the first Pakistan constitution was finally effected.

This calm, reasoned account, however, really ends with the death of the Quaid-i-Azam, Muhammad Ali Jinnah, in September 1948; it begins with the 1946 Cabinet Mission, although two essentially historical chapters precede, including one on the 1940 Pakistan Resolution.

The author has been careful in his use of sources and has bent over backward at times to be fair. He has taken great pains to cite corroborating testimony whenever his version differs from that advanced by Indian or British writers, and *Mission with Mountbatten* by Alan Campbell-Johnson, Lord Mountbatten's press secretary, is frequently cited, as well as the life of Gandhi by his secretary, Pyarelal. No new information appears in this balanced and discerning account of the Pakistan position. If a villain emerges he would seem to be Sardar Patel, a vigorously nationalistic Indian member of the Partition Council, although Gandhi, Nehru, and Mountbatten all receive some measured whacks. There is, in fact, little of the heroic in this tale, and on the Indian side much of the bitter and vindictive. It is a story of obstruction engendered to save a dream—the Congress dream of a united India, the re-creation of a world that never was—and of the many and perverse forms that obstruction took. Finally, it is the story of the triumphant growth of Pakistan despite the best efforts of the Congress leaders to dispose of it. Five of the book's seventeen chapters are concerned with the problems of the new state, many of which were themselves the direct result of the inability of the government of the republic of India to accept the idea that Pakistan could be a viable nation.

This is an important book for any student of the subcontinent; it is an effective, logical presentation and defense of the Pakistan viewpoint so far as partition was concerned and the account of a perceptive administrator of the struggle of the fledgling state to stay alive despite the most unfavorable circumstances.

Tufts University

FREELAND ABBOTT

TIBET: A POLITICAL HISTORY. By *Tsepon W. D. Shakabpa*. (New Haven, Conn.: Yale University Press. 1967. Pp. xii, 369. \$10.00.)

FOUR LAMAS OF DOLPO: TIBETAN BIOGRAPHIES. Volume I, INTRODUCTION & TRANSLATIONS. Edited by *David L. Snellgrove*. (Cambridge, Mass.: Harvard University Press. 1967. Pp. ix, 302, 46 plates. \$14.50.)

TSEPON Shakabpa's book is a remarkable presentation of the political history of Tibet from earliest times to the present. After a short survey of the specific geographic, social, economic, religious, and cultural features of Tibet, the author turns to his main objective, an account of the development of and changes in the political structure over the centuries and the growing involvement with the neighboring countries. In the discussion of the military power of the early Tibetan kings, the rivalries of the influential clans and their establishment of hegemonies that also exerted a tremendous influence on the intellectual and spiritual life of the Tibetans, the emergence of the Dalai Lamas and the relation

of Tibet with Nepal, India under British rule, and China, the author provides the reader with much new and important information by utilizing Tibetan materials that have usually been ignored by writers on Tibet, either because these materials were inaccessible or the writers did not know Tibetan sufficiently. As a former high-ranking official, the author also had access to government documents; their utilization, together with the author's vast knowledge of Tibetan historical traditions, adds to the value of this book which comes as a timely corrective to the prevailing misconception about Tibet and its history. In this excellently balanced and highly reliable account of Tibetan history the author displays his training as a diplomat, particularly in his treatment of the most recent events in Tibet, by remaining silent about the rather ignominious role India played under Nehru in ignoring Tibet's peculiar political status.

Dr. Snellgrove's book, by contrast, is essentially concerned with the cultural aspect of a remote area in what formerly was part of western Tibet, but now is a small and isolated region in northwestern Nepal, as reflected in the biographies of four lamas. These biographies, which clearly reveal the different character traits of the lamas, were composed by devoted disciples, although "the material used was all explicitly autobiographical." Three of these biographies belong to the fifteenth and one to the seventeenth century; all are valuable sources for the daily life of the people in Dolpo since religion provided all their cultural and educational needs. In the introduction to his translation Snellgrove traces the growing isolation and cultural as well as economic decline of the Dolpo region. He also gives a rather succinct outline of the prevailing philosophy and religion without which many passages in the biographies would not be easily understandable. The account of his personal experiences in Dolpo not only reveals his enthusiasm for the Tibetan people, but also substantiates the statements in the biographies. This is a well-written and thoroughly engaging study of a rapidly declining and disappearing culture.

University of Saskatchewan

H. V. GUENTHER

EARLY INDONESIAN COMMERCE: A STUDY OF THE ORIGINS OF
SRIVIJAYA. By *O. W. Wolters*. (Ithaca, N. Y.: Cornell University Press.
1967. Pp. 404. \$8.75.)

This is a significant contribution to the study of the early history of Southeast Asia. After a wide-ranging examination of possible sources, the author arrives at tentative answers to some puzzling problems of the early history of the first empire in Indonesian history. The rapid rise of Srivijaya is the mystery that the author primarily seeks to solve. By A.D. 700 it had become the foremost commercial power in western Indonesia and had acquired an outpost on the Malay Peninsula that gave it a commanding position on the straits. Why should Palembang in southeast Sumatra, without a rich hinterland and situated a considerable distance from the Straits of Malacca, become a major entrepôt and power in the region?

Professor Wolters suggests that the answer lies in a number of factors. The origins of Srivijaya are found in the circumstance that the area was "the cradle of the coastal Malays as they appear in history," a people who were exceptionally

skilled in seafaring. It has been assumed that the Indian, or "Persian," trade had been carried on exclusively by Indian traders, but Wolters is convinced that the Malays were active in it. "The expansion in trade at that time," he concludes, "was an indigenous and not an Indian achievement." Later trade with China developed, and Srivijaya became the flourishing intermediary of the trade between India and China. Gradually Indonesian products became prominent in the trade with China, and all harbors capable of sending ships to China began to engage in this trade. To protect its commercial position Srivijaya sent expeditions against Kedah and other rival trading centers and reduced them to vassals. Wolters ascribes the decline of Srivijaya to the expansion of Chinese shipping and the revolt of the vassals, who, determined to engage directly and independently in the lucrative trade, welcomed foreigners to their centers.

This study is based on the scanty evidence found in fragmentary, chiefly Chinese, documents. Its conclusions are hypothetical, but it is the best that can be done under the circumstances. The handicap of the paucity of primary sources for this period may be overcome by archaeological research in Sumatra.

University of Kentucky

AMRY VANDENBOSCH

LAND TENURE IN VILLAGE CEYLON: A SOCIOLOGICAL AND HISTORICAL STUDY. By *Gananath Obeyesekere*. [Cambridge South Asian Studies.] (New York: Cambridge University Press. 1967. Pp. xi, 319. \$13.00.)

THERE is a tendency among anthropologists to look to the past of the societies they study for explanations of contemporary social and cultural arrangements. This seems to reflect, in part, a drift from the synchronic and contemporary focus of structural-functional analysis, but it also is a consequence of the changing subject matter of anthropology. The people studied increasingly are peasant folk who constitute parts of larger, often highly civilized, aggregates maintaining important connections in space, with social and cultural cosharers, and, in time, with recorded pasts.

This study of a village in the southern province of Ceylon makes full use of the various pasts including the legacy of the pre-1815 Kandyan "feudal" system as a model for social relations at present, the impact of Roman-Dutch law upon inheritance, British revenue innovations during the nineteenth century, such as the "Grain Tax" and regulations governing the partition of landed estates, and such postindependence factors as political office and influence in the development of new kinship arrangements associated with landholding. This time dimension and a lively sense of the interdependence of landholding and social structure permit Dr. Obeyesekere to bring to light two important and heretofore ignored institutions: the *vāsagama*, a kinship group with ancestral rights in a portion of village land; and the *pelāntiya*, the modern kindred led by men of achieved status and prestige. The operation of these and related peasant institutions that affect land tenure certainly suggests an important approach for the study of land tenure systems in nineteenth- and twentieth-century India where, for example, in Bengal just as in the Ceylonese village of Madagama, the complex levels of tenurial rights over economically irrelevant landholdings emphasize the need for more systematic study of status and prestige factors. It is in the study of such factors,

however, that the monograph is often confused and ambiguous. The well-known Kandyan "feudal" system is discussed at some length, fitted out with some newer terms from contemporary sociology, but the reader is left to wonder whether this "feudal model" provides an empirical, emulative scheme for contemporary villagers. If it does, we are not shown how. Nor is the Kandyan system very clearly a postulational model, that is, a system of rules followed by the modern villagers of Madagama. The introduction of Weber's concepts of "status honor," "status group," and "status stratification" are, similarly, not very helpful in explaining why the tenurial and social arrangements in this part of Ceylon moved "rational men" to "invest" in the "share market" of landholdings of the village. If it is "status," then is it part of a defensive peasant strategy or the maximizing activity of upwardly mobile "outsiders"?

These problems and others are to be expected in the field of historical and contemporary peasant studies, for, notwithstanding the achievements of European medievalists, studies of peasant communities and cultures are still in their infancy. This volume certainly moves us along with both its empirical, descriptive material and its theoretical formulations.

University of Hawaii

BURTON STEIN

VIETNAM: A DRAGON EMBATTLED. Volume I, FROM COLONIALISM TO THE VIETMINH; Volume II, VIETNAM AT WAR. By *Joseph Buttinger*. (New York: Frederick A. Praeger. 1967. Pp. x, 663; 667-1346. \$18.50 the set.)

THIS is a continuation of the political history of Vietnam that Joseph Buttinger began in his *The Smaller Dragon*. That earlier work is much smaller in scope, though it covers the long period from Vietnam's semilegendary beginnings to 1900, whereas the present study takes two volumes to deal with only about a century, spanning the colonial period and the struggle from 1945 through the Diem regime.

No reader who has the perseverance to peruse the 1,257 pages of this study can fail to be impressed by its author's industry. Buttinger has amassed and organized an extraordinary amount of data, much of which has never before been made available in English. His 445 pages of notes and his bibliography are, to me, the most interesting and informative part of the work. Buttinger's motivation in undertaking this colossal task is clearly the same as that which made him, formerly, a leader of democratic socialism in Austria. Many of his statements are unexceptionable, notably those attesting to the strength of Vietnamese nationalism. And his championship of the sorely tried and courageous Vietnamese people and of their aspirations for independence cannot but call forth the reader's sympathies.

Yet to describe Buttinger, as some of his admirers have done, as the foremost American historian of Vietnam, and to call his works scholarly, is to overlook definite weaknesses. That his books should contain some factual errors is probably inevitable in volumes of their scope and length. To find objectionable certain of his "purple" passages is, moreover, a matter of individual taste in style. Nor can Buttinger be held responsible for not having firsthand knowledge of Vietnam during the colonial period. On the other hand, his ignorance of the Vietnamese

language is a major handicap to his scholarship, as is his apparent failure to have consulted personally any of the numerous Frenchmen who participated in the events he describes or others who have had long experience in the country. Their views might have mitigated some of Buttinger's categorical assertions.

The author makes no attempt to approach his subject objectively. His is a labor of love and also one of righteous indignation. He seems to regard the drama of Vietnam as a sort of medieval morality play, in which the forces of evil—as represented by French policy and Communism—are arrayed against the “good” Vietnamese people. France's cardinal sins have been, in his eyes, first its oppressiveness as a colonial power and second its attempt to disguise a policy of force and self-interest as a crusade against Communism. He has particularly harsh words for the French non-Communist Left parties because they sacrificed their principles to support the Rightist “colonial party” for reasons of domestic political expediency. His indignation is the more intense because he views such a compromise as the defect of all moderately successful socialist leaders of our time. In his view, France is mainly responsible for the existence of dictatorships today in North and South Vietnam, largely because it forced the non-Communist nationalists to choose between collaborating with a colonialist administration and joining the Communist Viet Minh. In his dislike of all authoritarian regimes, Buttinger finds Ho Chi-Minh only slightly less objectionable than Ngo Dinh Diem.

Buttinger has made a valiant effort to provide the background needed by a confused and uninformed American public if it is to begin to understand the complicated situation in Vietnam. The definitive history of that country, however, remains to be written.

University of California, Berkeley

VIRGINIA THOMPSON

VIETNAM: THE ROOTS OF CONFLICT. By *Chester A. Bain*. [Spectrum Book.] (Englewood Cliffs, N. J.: Prentice-Hall, 1967. Pp. viii, 184. Cloth \$4.95, paper \$1.95.)

THE paramount problem facing the US government and the American people is the current war in Vietnam—the government searching for methods to conclude the holocaust; the American people desiring to know the basic reasons back of US involvement in that land. Tracing Vietnam's development from its earliest beginnings until the Franco-Vietnamese colonial war broke out in late 1946, the author presents a thumbnail sketch of the history of Vietnam to bring about a better understanding of the Vietnamese people and their tragic conflict. He provides an assessment of the influence of Vietnam's complicated cultural heritage, with its tangle of Confucianism, Buddhism, Catholicism, and native spirit lore; an evaluation of Chinese social and political institutions; and a sharp analysis of the Western concepts of individualism, progress, and social mobility, all of which are related to the course of events in modern Vietnam.

Following the Battle of Dien Bien Phu in 1954 when the French were defeated, Vietnam became a divided country north and south of the seventeenth parallel, the military demarcation set by the French and Communists at Geneva. The chapter on “The Communist North” is a discussion of the DRV's First Three Year Plan (1955–1957) aimed at rehabilitating agriculture, industry, and trans-

portation in preparation for collectivization. Information is also given about the second plan (1958-1960), which brought about a new constitution and proclaimed Vietnam a single nation. Having consolidated Communist party control, the DRV turned southward. Reunification was vital to the economy of the North because the failure in food production jeopardized the whole development program there. By winning the South, the DRV hopes to feed the people of the North and to secure rice and rubber for necessary export as well as for the support of further industrialization. It is fairly well established that without the South the DRV is virtually reduced to perennial mendicancy. When preparing this part of the discourse, the author had available large numbers of translated Vietcong documents and Communist writings.

The closing chapter, "Efforts for Peace," is valuable not because it points the way whereby peace will come but because it outlines in documentary style the various proposals that have been extended by the Communists of the North, by the government of South Vietnam, and by the US.

The author, at present an officer of the US Information Agency and formerly stationed in South Vietnam, has produced not a profound treatise but an acceptable background book with which one can probe further into the problem that is facing the US and Vietnam today.

Library of Congress

CECIL HOBBS

NEW ZEALAND LETTERS OF THOMAS ARNOLD THE YOUNGER, WITH FURTHER LETTERS FROM VAN DIEMAN'S LAND AND LETTERS OF ARTHUR HUGH CLOUGH, 1847-1851. Edited by *James Bertram*. ([Auckland:] University of Auckland; New York: Oxford University Press. 1966. Pp. xlvii, 257. \$12.50.)

EARLY in 1848 Thomas Arnold, the second son of the famed Dr. Arnold of Rugby, arrived in New Zealand, where he hoped to put behind him an unhappy love affair, to escape the growing clutter, restrictions, and conservatism of English society, and to experiment with his own ideas of education. He remained but two years, discovering the New Zealand bush to be less romantic and hospitable than he had expected, and moved on to Van Diemen's Land where, as inspector of schools, he was able to institute at least some of the reforms he envisioned. While in Hobart he married Julia Sorell, belle of the colony's garrison society and daughter of a former governor. He also became a convert to Roman Catholicism and, appealing to J. H. Newman, then in Dublin, left with his wife, first for Ireland and eventually for England, to experience in time a relapse from Catholicism and then a return to it. Throughout this time, in common with many Victorians, Arnold was an indefatigable letter writer; he wrote with restraint but also with fullness of his impressions of the Colonial Office (where he worked as a *précis* writer before going to the Antipodes), of New Zealand and Tasmania, and of his spiritual quandaries.

The results of this correspondence have been brought together and admirably edited by Professor James M. Bertram. Bertram presents us with sixty letters, thirty of which originated in New Zealand and twelve in Van Diemen's Land, together with several useful, terse, and fully adequate appendixes on the so-called

Equater Letters which Arnold sent to J. C. Shairp, on Julia and the Arnold family, and on Arthur Hugh Clough, who as Arnold's close friend was the recipient of eleven of the letters printed here and who, in turn, wrote eight of those received by Arnold. Well-chosen illustrations, sensible and sensitive editing, and a brief but informative introduction help make this volume an extremely useful contribution to our understanding of the Victorians. It is regrettable that Arnold does not appear to have been especially thoughtful about the cultures of the people among whom he traveled, being content to dismiss the Maori, for example, with a slighting and generalizing reference after but two months' acquaintance, and while the picture of Antipodean society that emerges is an interesting one, it adds little to what we already know and provides a poor contrast with the insights of that other great Victorian down under, Samuel Butler.

Yale University

ROBIN W. WINKS

PARLIAMENT FACTIONS AND PARTIES: THE FIRST THIRTY YEARS OF RESPONSIBLE GOVERNMENT IN NEW SOUTH WALES, 1856-1889. By *P. Loveday* and *A. W. Martin*. ([Carlton:] Melbourne University Press; New York: Cambridge University Press. 1966. Pp. xiii, 207. \$10.50.)

IN this well-written, lucid, and significant pioneer study for historians and political theorists, old assumptions and conclusions about the confused political developments between the coming of responsible government in New South Wales in 1856 and the appearance of a true party system in 1889 are proved misleading.

After an excellent analysis of the liberal-conservative division of the 1850's, the authors trace the emergence of the faction system, which featured the power struggles of rival leaders such as Parkes, Martin, Forster, Cowper, and Robertson. Small, closely knit bands of followers furnished these leaders with the critical mass necessary to form stable parliamentary majorities, but a faction "lacked any well-defined body of principle or belief." For analysis of the factions the authors draw on two types of evidence: relations and allegiances of members as described in the press, debates, and letters and memoirs; and the voting record of members in Parliament. Useful statistical tables show the various changes. The distinction between a "steady" and an "unsteady" supporter is, unfortunately, not quite clear. The chief methods used by leaders of factions to win and hold power were maneuver and intrigue. Incidents and crises in which ministries were made and unmade are studied. The practice of caucusing, which committed a vote before debate, became common. Members of Parliament, often bound by personal respect and affection for a leader, many times voted independently of their electorate's wishes. Interesting conclusions are the development of the cabinet system long before the emergence of political parties, the complete absence of a spoils system, the importance of coalition as a maneuver, the logic and stability of the faction system, the surprising durability of some of the ministries, the impartial attitude of governors such as Denison, Young, and the Earl of Belmore, and the desperate nature of the struggle for office, since members were unpaid, and ministers well paid.

The authors admit the speculative nature of some conclusions and at times

define and categorize too confidently, when the study in reality is breaking ground in a difficult area.

University of California, Irvine

SAMUEL CLYDE McCULLOCH

Americas

HISTORICAL SCHOLARSHIP IN THE UNITED STATES AND OTHER ESSAYS. By *W. Stull Holt*. (Seattle: University of Washington Press. 1967. Pp. xii, 184. \$6.95.)

THIS volume of essays clearly identifies Stull Holt as one who carries his responsibilities as a historian with dignity and understanding. A brief introduction, written by other hands, outlines his career and bears special tribute to his gifts as a teacher. Brought together on the occasion of Holt's retirement as professor of history at the University of Washington, these essays were written at various times over a period of a quarter of a century.

The largest section is devoted to papers on "Historical Scholarship and the Historical Profession," reflecting Holt's interest in both the craft and craftsmanship. To him, the last quarter of the nineteenth century and the early years of the twentieth form the most significant period in American historiography, when, as he points out, "two distinct forms of historical writing, the broad canvas and the miniature, flourished side by side." The Johns Hopkins tradition is strong in Holt, and the significant figure of Herbert B. Adams looms large in his discussion of the period. Holt refers with obvious approbation to his action, when, in conversation with Jameson, Adams told him that he had planned a large work on the relation of church and state, but never got to it because it would prevent him from devoting himself to his students. By carrying his account into the present century, Holt has really given a review of modern American historiography. A brief paper on "Who Reads the Best Histories," stimulated by a list of Professor John Caughey, presents Holt's selection of the twenty best histories and biographies. His conclusions as to who reads the best books, based on figures of sales, is rather amazing. We wonder whether the great increase in the publication of paper-bound reprints in the thirteen years that have elapsed since the article appeared would change his conclusion. The last three essays are, in effect, Holt's evaluation of large studies made by the Committee on Historiography of the Social Science Research Council and the Committee on Graduate Education of the American Historical Association.

The essays in the second part reflect Holt's interest in diplomatic history and particularly in the question of American security. Of special value is his discussion of Professor C. Vann Woodward's concept of "free security" as he developed it in his paper "The Age of Reinterpretation." Granting the importance of free security, Holt argues that the source of free security during the greater part of our national life was to be found "not in the accidents of geography, but in the accidents of history."

A final section includes his presidential address at the 1952 meeting of the Pacific Coast Branch of the American Historical Association urging the im-

portance to the historian of the process of urbanization and pointing out some significant phases for investigation. The fifteen years since that address have heavily underscored the soundness of Holt's judgment.

These essays are eminently readable and as wide in their appeal as Holt is in his scholarship, his professional concern, and his human understanding.

George Washington University

ELMER LOUIS KAYSER

MADOC AND THE DISCOVERY OF AMERICA: SOME NEW LIGHT ON AN OLD CONTROVERSY. By *Richard Deacon*. (New York: George Braziller. [1967.] Pp. xiii, 269. \$5.00.)

THE pre-Columbian discovery of America from Europe has been a favorite subject of authors for many years. Egyptians, Phoenicians, Greeks, Irish, Welsh, Vikings, and others have all had their proponents and defenders, but only the Vikings have been substantiated by archaeological evidence and documentation. The case of Madoc the Welshman is presented in this book, and, to me at least, it does not appear to be a strong one.

Briefly, the legendary story implies that Madoc, the son of a Welsh chieftain named Owain Gwynedd, discovered America in 1170, 322 years before Columbus. He is said to have returned to Wales and sailed a second time with more colonists. The legend received additional impetus from the accounts of various North American travelers and other individuals who claimed to have encountered tribes of Indians who spoke Welsh. Usually these tales involved a Welsh-speaking person who sometimes talked his way out of death or torture by conversing with his captors in Welsh. Geographical location of the tribe of Welsh-speaking Indians has ranged from Carolina to Oregon and from the Gulf of Mexico to the Missouri. Like a will-o'-the-wisp it is always tantalizingly beyond reach. Tribes as disparate as Cherokee, Comanche, and Kutenai were suggested as descendants of Madoc and his Welshmen but finally for various reasons, mostly one suspects because they became extinct, the identification settled on the Mandans. There are those, however, who are convinced of the authenticity of the voyage. In 1953 the Virginia Cavalier Chapter of the Daughters of the American Revolution erected a tablet to Madoc at Mobile Bay.

The author has gone to some pains to investigate various aspects of the Madoc voyages and the Welsh Indians. It is not easy to identify the hero or even his father in the mixture of Welsh history and poetry. The types of vessels available to a Welshman in the twelfth century are but indefinitely known; the evidence that Columbus had heard of Madoc is slim; and the travelers' tales of Welsh Indians have an invariably fishy flavor.

All in all, occasional resemblances between Mandan and Welsh material culture and word coincidences are not enough to convince the conservative scholar of Madoc's voyages or the existence of his Indian descendants. The book, which has occasional lapses (neither the Osage nor the Delaware, for example, is Iroquoian), is entertaining, and it is convenient to have the Madoc tale summarized in one volume.

Peabody Museum of Salem

ERNEST S. DODGE

FROM PURITAN TO YANKEE: CHARACTER AND THE SOCIAL ORDER IN CONNECTICUT, 1690-1765. By *Richard L. Bushman*. [Publication of the Center for the Study of the History of Liberty in America, Harvard University.] (Cambridge, Mass.: Harvard University Press. 1967. Pp. xiv, 343. \$8.95.)

THE "land of steady habits" proved a misnomer for Connecticut colony during the period of Professor Bushman's study. Beginning about the last decade of the seventeenth century, he examines the principal forces that he feels upset the colony's rigid and static social order and that transformed the well-ordered inhabitants into independent and individualistic "Yankees." He stresses, primarily, the economic factors that contributed to undermining the original establishment. Traditional corporate landowning practices were replaced by individual proprietorship and land speculation; a new group of merchants emerged to challenge the standing order on issues such as paper currency; and a general spirit of covetousness "destroyed the peace of society."

Disruptive religious developments are described in the context of growing minorities of dissenters and, more importantly, in the numerous contentions between rebellious congregations and the ambitious assembly-supported Congregational ministers. From this perspective, the climactic Great Awakening is viewed primarily as the culmination of a "contest between piety and order"—the New Lights representing the former position, and the Old Lights representing the latter. The revival split the established churches, but in its wake gains were made in personal, political, and religious liberties. Bushman's final chapter, where he surveys this prevailing "Yankee Spirit," concentrates on the Connecticut social order of 1765, which had restructured the attitudes of the common man toward government, religion, and even society itself.

This book represents a valuable contribution to an understanding of the social and economic history of colonial New England. The author has done a superior task of organizing his material and provides helpful maps, graphs, and an informative critical bibliography. His writing is clear, and his study is accurately documented. He has selected, on the whole, the most relevant sources, although an examination of pertinent SPG manuscript material and the Lambeth-Fulham Palace Papers could have broadened his discussion of the Anglican minority in Connecticut. While his interpretations are largely incisive, there are a few instances of oversimplification. The fact, for example, that these transformed Connecticut Yankees still retained a significant heritage from the past is well illustrated by Professor Edmund Morgan in recent works on Puritan political ideas and the Puritan ethic. Such exceptions, however, are infrequent and do not detract from this notable and perceptive study.

Bradley University

SHELDON S. COHEN

CHURCH AND STATE IN FRENCH COLONIAL LOUISIANA: POLICY AND POLITICS TO 1732. By *Charles Edwards O'Neill*. (New Haven, Conn.: Yale University Press. 1966. Pp. xii, 315. \$10.00.)

ALTHOUGH he has presented a good synopsis of political and social develop-

ments, Professor O'Neill is primarily concerned with the interaction of Church and state policy during the first three decades of French colonization in the lower Mississippi and Gulf area. Whereas the English believed that southern Indians would be won by economic ties to Carolinian traders, French policy as set down by Lemoyne d'Iberville emphasized the vital importance of the Church in holding the Indians loyal to the French and stopping the English. The Church, the author contends, was from the first an integral part of the scheme of colonization, not a supplementary appendage.

O'Neill weaves an intricate narrative from thorough research into archival materials, and occasionally one gets lost, but what emerges clearly enough is the story of continuously lost opportunities, feuds, conflicts of personality, and unfulfilled hopes. Churchmen clashed with churchmen; Jesuits, Capuchins, Carmelites, and seminarians from Quebec and Paris fought over ecclesiastical powers and mission territories; and St. Vallier, bishop of Quebec, unable to communicate with Mobile and New Orleans, refused to allow division of his sprawling diocese. Despite heroic individual efforts, the result was an ineffectual ministry to settlers and Indians alike.

The same division of authority between military commandant, financial commissary, and the clergy that retarded French Canada hampered Louisiana. The principal antagonists were commandants Bienville and LaPerier, commissaries Duclos and LaChaise, Vicar-General Davion, Capuchin leader Père Raphael, Jesuit Beaubois, and LaVente, pastor of Mobile. Here again the issues were personalities and prerogatives, not fundamental differences about functions of Church and state. Unable to work together, the colonial leaders unsuccessfully relied on distant and often disinterested Paris for a solution to their problems.

After thirty years neither Church nor state prospered. In all Louisiana there were only 2,500 settlers and several dozen clergy. France had set too great a task for too few men with too little support. Only during that brief period between 1717 and 1720 when John Law placed Louisiana in the hands of the Company of the Indies, abandoned religious ties to the Indians and emphasized trade instead, and sent hundreds of settlers with priests to Louisiana did both colony and Church expand.

University of Virginia

DAVID ALAN WILLIAMS

ROBERT JOHNSON: PROPRIETARY & ROYAL GOVERNOR OF SOUTH CAROLINA. By *Richard P. Sherman*. (Columbia: University of South Carolina Press. 1966. Pp. xii, 203. \$6.75.)

ROBERT Johnson, proprietary governor of South Carolina from 1717 to 1719 and royal governor from 1731 to 1735, was one of the best provincial governors in the American colonial period. In recent decades leading authorities have given Johnson his due. David Duncan Wallace praised Johnson's "wisdom, moderation, and skill"; Robert L. Meriwether saluted Johnson's "most popular and most successful of the royal administrations" of South Carolina; and the late M. Eugene Sirmans deemed Johnson "the most remarkable politician in the colonial history of South Carolina."

Until the present work we have had no biographical study of Johnson. In this book Richard P. Sherman gives us a convenient, complete, reliable account of Johnson's gubernatorial years. The book's defect is its interpretive weakness, especially in regard to the royal years.

Sherman is more successful in his account of Johnson's proprietary career. These years featured elemental problems of survival against Indians, pirates, and the Spanish. Sherman relates Johnson's direct, self-reliant solutions with crisp competence. The more subtle challenges and the greater complexity of political and economic forces in the royal period of Johnson's rule are not handled as well by the author. For Johnson's royal governorship the briefer but more pointed account by Sirmans, in *Colonial South Carolina* (1966), a book with which Sherman's study will inevitably be compared, is preferable.

Sherman and Sirmans each present strongly favorable but clearheaded estimates of Johnson as royal governor; the difference between the two authors lies in Sirmans' greater penetration and grasp of the nuances. Space permits the citation of only one important and typical example: the controversy over paper money. Sherman sees the problem as one of simple conflict between opposing creditor-merchant and debtor-planter factions with Johnson somewhat mystifyingly gaining merchant support for a paper money solution. In Sirmans' more subtle analysis there are three factions: small farmers of debtor status; a narrow group of mercantile hard money men; and a swing faction of moderate paper money men composed of both merchants and planters. Johnson's triumph, says Sirmans, was to utilize the swing faction to obtain a paper money act that he then shrewdly balanced with a debt-suit law to the liking of the defeated hard money faction. In all this Johnson was crucially aided by the moderate merchant-politico, Samuel Wragg, who does not figure in Sherman's analysis. In short, Sherman's treatment of Johnson has greater breadth; Sirmans' has greater depth.

College of William and Mary

RICHARD MAXWELL BROWN

TRAVELERS IN TEXAS, 1761-1860. By Marilyn McAdams Sibley. (Austin: University of Texas Press. 1967. Pp. 236. \$5.00.)

THE trans-Mississippi West held a peculiar fascination for both Americans and Europeans throughout the nineteenth century, especially the Texas frontier preceding the Civil War. Though unencumbered by several pounds of camera equipment that modern tourists invariably carry, the literate traveler was rarely without a pen and notebook. His observations furnished the raw material for innumerable books about Texas. Among the more important nineteenth-century chroniclers were William Bollaert, Nicholas Doran P. Maillard, William Kennedy, Mary Austin Holley, Noah Smithwick, and Frederick Law Olmstead. It is upon their printed works that the author of the present volume relies most heavily, but by no means exclusively.

Marilyn McAdams Sibley has taken a better than average dissertation and turned it into a readable and useful volume. In *Travelers in Texas, 1761-1860*, she evaluates and synthesizes various accounts and impressions of Texas as seen by the traveler-writer. Some, like the Englishman Maillard, were especially

critical of everything they saw. Maillard's objective was to prevent British recognition of the republic, and his observations reflected a confirmation of preconceived ideas. Others wrote to capitalize upon the public's vicarious interest in travel, attract colonists, warn prospective settlers away, promote land sales, or, as in the case of Smithwick, to while away the tedium of old age. Regardless of motives or literary abilities, their writings were seldom dull.

Through the eyes of the traveler, particularly those who went to Texas after 1830, Sibley examines such topics as Indians, slavery, immigration, law enforcement, and the picturesque language of the frontier. Professor William R. Hogan covered all of these subjects in more depth a generation ago in *The Texas Republic: A Social and Economic History* (1946), but for a much briefer period. There is, indeed, little in the present volume that has not been told already. This is not to say that the author has performed a useless task. Her "Cultural Essay on Sources" at the end reflects a thorough knowledge of some two hundred basic works and is a model of concise, interpretive writing. When done properly, the critical essay on sources is far more usable than the mere listing of titles. Sibley does both, for she also includes a formal bibliography.

This book will appeal to readers and scholars outside the region as well as within. It is by no means definitive, but then it was not intended to be.

University of Toledo

W. EUGENE HOLLON

PHILIP FRENEAU: CHAMPION OF DEMOCRACY. By *Jacob Axelrad*. (Austin: University of Texas Press. 1967. Pp. xii, 480. \$6.50.)

Philip Freneau is best known as "the Poet of the American Revolution" and is also considered by some as the first American Romantic poet. His contemporaries, however, probably regarded him primarily as a contentious misfit who used his pen to find fault with virtually everything.

Despite the advantages that Freneau had as a youth—a good library at home, schooling at an excellent Latin school, and finally three years at Princeton, where he was the roommate of James Madison and a classmate of Hugh Henry Brackenridge—he found it difficult to establish himself. Throughout his life, he moved from one position to another. In the two decades after his graduation from Princeton, for example, he read law, studied for the ministry, tried his hand at teaching twice (he stayed at one school less than two weeks), was a propagandist in New York City for the rebel cause, became a sailor, went to the West Indies for several years, enlisted in the New Jersey militia, served on a blockade-runner, became a soldier again, sailed on a privateer, was a prisoner of the British, and spent some time as a postal clerk.

Perhaps it was his Huguenot background, perhaps it was the family financial troubles, or perhaps it was his lonely moodiness that made him use his literary talents to champion the cause of democracy. Even while he was still at Princeton, Freneau became a critic of British rule. And for his college commencement in 1771, he wrote, in collaboration with Brackenridge, a poetic dialogue, "The Rising Glory of America." In this, as well as in subsequent poems, essays, and newspaper articles, his theme, usually more emotional than factual, was that the common man must be the beneficiary of the break with the mother country and

of the new government of the United States. Thus, unlike most non-New England writers of the period, Freneau was a liberal.

Following the war, his first attack was upon the Pennsylvania Constitution; to him the unicameral legislature would become the stronghold of the aristocrats. As editor of *The National Gazette*, he became highly critical of the growing conservatism of Washington's administration and an increasing advocate of Jeffersonian democracy. The articles, both prose and poetry, that appeared in the numerous newspapers on which he served were written so effectively that they helped to arouse popular interest in the causes he championed. Contemporary opinions naturally varied. Jefferson declared that Freneau had prevented the Constitution from "galloping fast into monarchy"; to Washington, on the other hand, he was a rascal, and to Hamilton, a liar.

The author generally writes well, but he fails to make the reader aware of Freneau as a person. Too much emphasis has been placed upon him as a literary figure, and not enough as a "champion of democracy." While Axelrad has made full use of primary material on Freneau, the secondary sources included in his footnotes leave much to be desired. This volume is much better as a treatise on the literary scene than it is on the historical approach for the period covered.

Syracuse University

O. T. BARCK, JR.

THE AMERICAN REVOLUTION RECONSIDERED. By *Richard B. Morris*. (New York: Harper and Row, 1967. Pp. xi, 178. \$5.00.)

IN these four extended essays, excellent revisionist insights are sprinkled amidst somewhat more routine historiographical orthodoxy. In its origins and common purpose, Morris believes, the Revolution was an anticolonial war fought for independence and national identity; yet "it was also marked by liberative currents, class conflicts, and egalitarian urges." The author uses his vast knowledge of the period with facility in buttressing points of his own or challenging theses of others. His familiarity with local episodes and biographies makes his generalizations lively and usually persuasive. Occasional stylistic lapses and repetition are more than balanced by brilliant phrasemaking and striking analogies to contemporary affairs.

Chapter I, an overview, is the least satisfying because it is uneven. Although shrewd, for example, in assessing the imperial school, Morris is overly pejorative toward the Namier influence. Chapter II compares the American and French Revolutions judiciously. The author revitalizes Becker and Jameson in finding that a considerable social upheaval took place here. He has striking remarks on the fear of the patriot Whigs of the lower orders in society and rejects the notion that the colonies were rather classless and homogeneous on the eve of revolution. (It is ironic, however, for Morris of all scholars to refer slightly to "diggers in county court houses.") There is much good sense in his examination of landholding and loyalism and the significance of entails. Chapter III treats wartime diplomacy, contending that the French military alliance was almost as much a liability as an asset, that the American commissioners should not have trusted Vergennes, and that John Jay was wise to be so suspicious all around. Morris uses material from *The Peacemakers* effectively, especially in reconstruct-

ing the Necker-Mallet-Mountstuart secret negotiations in 1780. The final chapter contends that there really was a critical period and that the movement to establish the Constitution had broad support at the popular level and did not involve a counterrevolution.

As the bicentennial gets under way we should be grateful that laymen will have available such a brisk, up-to-date synthesis that is also exceedingly independent in judgment. The book does, however, present puzzling problems of consistency that may perplex the general reader. Morris explicitly sets the revolutionary era in a historical framework of both consensus and continuity. Yet he is often at pains to stress the lack of consensus as well as significant discontinuities. Similarly, while the preface repudiates writers who depict the Revolution "as a conservative movement, essentially political in its objectives," the author momentarily takes this view himself (p. 39). Actually these are not conceptual difficulties so much as ones of precision in essays that are engaging and forthright. Perhaps Morris' ambivalence reflects ambiguities inherent in the American Revolution itself.

Cornell University

MICHAEL G. KAMMEN

THE WAR IN THE NORTH: AN INFORMAL HISTORY OF THE AMERICAN REVOLUTION IN AND NEAR CANADA. By *Donald Barr Chidsey*. (New York: Crown Publishers. 1967. Pp. 214. \$3.95.)

ALTHOUGH Donald Chidsey's book is admittedly popular history rather than a scholarly study, this work falls far short of meeting minimum standards even when measured in terms of its own genre. The title, for example, is a misnomer; the volume does not cover all military operations in or near Canada throughout the American Revolution. Over three-fourths of the book is devoted to two major campaigns—Arnold's march north to Quebec and Burgoyne's drive south to Saratoga—leaving little space to mention other important developments in the northern theater of operations. The same charge of inaccuracy can be levied against Chidsey's use of sources. Although the author claims he is "most deeply indebted" to diaries of participants for his information, he has actually relied upon two secondary sources: Allen French's *First Year of the American Revolution* and Hoffman Nickerson's *Turning Point of the Revolution*. Both these works are respectable, but their findings are distorted to a degree by Chidsey's overly dramatic presentation of events.

Chidsey, a biographer and novelist, writes in a racy style more suited for fiction than factual history. Benedict Arnold appears as a "slick city feller" whose "eyes were squints of vinegar, his mouth . . . a steel-toothed trap"; Horatio Gates as "a tousled, nearsighted man, peery, a throat-clearer"; and Sir John Burgoyne as "a butterfly, a wit." Such characterizations are based as much on imagination as historical investigation.

The book has numerous errors, major and minor, and is full of faulty generalizations about British military figures. Burgoyne was helped, not hurt, by his marriage into the Derby family early in his career; he is reputed to have made the remark about a British attack in the Boston siege in order to gain more "elbow room," but we cannot be as certain as Chidsey is about the state-

ment; and Albany, not Ticonderoga, was Burgoyne's main objective in his campaign. The view of Lord George Germain is outdated and fails to take into account Gerald S. Brown's recent biography. Finally, the understanding between Burgoyne and Howe regarding the Saratoga campaign was not as clear as Chidsey represents it, and Burgoyne was not given inflexible orders to advance to Albany. Mistakes of this sort make the book useless to the scholar and a source of misinformation to the general reader.

The popularizer has a proper place in writing about America's past, but like every student of history he must do his homework first.

Clark University

GEORGE ATHAN BILLIAS

THE INVENTION OF THE AMERICAN POLITICAL PARTIES. By Roy F. Nichols. (New York: Macmillan Company. 1967. Pp. xii, 416. \$8.95.)

As we would expect from Professor Nichols, who has devoted a distinguished career to the study of American political history, a number of themes run through his general survey of the history of American politics prior to the Civil War, only a few of which can be noticed in a brief review. One theme, developed through a survey of colonial American political practices and their English origins, is that the basic patterns of American political behavior are "both ancient and not indigenous to the United States." Continuity from the English and colonial pasts is stressed, and a process of building on experience with a logical, almost inevitable, transition from one period to the next is depicted. Another theme is that American politics has been characterized by a large degree of improvisation, particularly under the demands of working out means to operate the federal system.

The central thesis of the book is that the present two-party system was not completed as a party system deserving of the name until the 1850's, when national nominating conventions with convention-appointed national chairmen, national central committees, and systems of party financing finally matured. This process had its beginning in the Jacksonian period with the introduction of the national nominating convention. It was not until the Jacksonian era that "the realities of American politics as they are now defined first begin to be discernible. Most of the pre-Jackson political experience differs from, more than it resembles, modern practice." Thus, the Federalist-Jeffersonian period is seen in much the same light as earlier periods of conflict; just as patriots organized to achieve independence and Federalists plotted to bring about the adoption of the Constitution, so the Jeffersonians organized to drive the Adams-Hamiltonian Federalists from power. Like these earlier organizations for specific purposes, the Jeffersonian party was a temporary phenomenon. The 1790's are not seen as partisan, nor the Jeffersonian triumph in 1800 as significant, largely because a permanent system of parties did not result. This requirement of permanency leads Nichols to regard all parties prior to the 1850's as lacking in essential organizational features that relegate them to incomplete political mechanisms. While much of the thesis rests upon the definition of a party system, I find more satisfactory the interpretation advanced by Richard McCormick, who sees the Democrats and Republicans of the 1850's as forming the third American party system. But in

his broad sweep of time and place, Nichols provides much to be pondered and further explored.

University of Missouri

NOBLE E. CUNNINGHAM, JR.

NATURE'S NATION. By *Perry Miller*. (Cambridge, Mass.: Belknap Press of Harvard University Press. 1967. Pp. xvi, 298. \$7.50.)

As a collection of essays, twelve of them previously published and three not, *Nature's Nation* makes a companion volume to *Errand into the Wilderness*, Perry Miller's 1956 collection; as containing essays that examine ideas he would have considered, probably in the third volume of *The Life of the Mind in America* had he lived to write it, the present book twins also with that one. Seven of the present essays are studies in Puritanism, or at least draw heavily from the author's unique learning in that field. The others, which are devoted to problems of nature, evangelicalism, Romanticism, and nationalism in American expression, draw mostly from the nineteenth century; occasionally they reach back to colonial beginnings or come forward to present concerns. The pieces not printed earlier—"An American Language," "Romance and the Novel," and "Sinners in the Hands of a Benevolent God"—all belong in the far-ranging group.

The "sinners" who concerned Miller in that last and climactic essay were the American people. As men of letters and churchmen have turned away from Calvinism as a system, he points out, they have nonetheless retained a sense of need for regeneration; they, and Americans generally, have comforted themselves by adoring the beauty of nature. But this has not met their spiritual requirements. Having let go the angry God of Jonathan Edwards, they have not yet discovered better than a half-god, in the nearby sublime.

After a foreword by Mrs. Miller, Professor Kenneth B. Murdock in an introduction urges, what the book's present-mindedness particularly justifies, that this volume is necessary to grasp Miller's thought as a whole; he pays a colleague's tribute to Miller as teacher, giving credit to his wit and involvement, his ad-libbing and his overstating, all parts of an exceptionally personal method. This illuminates a troublesome factor in Miller's writings, which is present again in this book. To illustrate, I believed, when I first read the essay "From the Covenant to the Revival," that it exaggerated beyond credibility the role of religious nationalism in achieving American independence. The new context, herewith, does not absorb the feeling of overstrain. Again, there must be more appropriate words than "divine determinism," or plain "determinism," which open a 1943 essay, to characterize the "Calvinist" understanding of events on this earth. The word forces the argument: "determinism," bearing the suggestion that history can be calculated by man from cause to effect, does not comport with a general Calvinist sense of God's sovereignty, and I doubt that it does with a Massachusetts Puritan one.

Yet Murdock's large estimation is right. American history in our time has had no scholar superior to Miller at reading the word afresh, nor at presenting new findings.

Johns Hopkins University

CHARLES A. BARKER

CONGRESS AND THE CONSTITUTION: A STUDY OF RESPONSIBILITY. By *Donald G. Morgan*. (Cambridge, Mass.: Belknap Press of Harvard University Press. 1966. Pp. xv, 490. \$8.95.)

PROFESSOR Morgan's study is a plea for Congress to repudiate the axiom that the Constitution is "what the judges say it is," and to assume its rightful role of coordinate constitutional responsibility. Using legislative case studies, Morgan illustrates congressional behavior and attitudes toward constitutional interpretation. The record since 1890, he concludes, indicates an alarming increase in congressional abdication of its proper role in such matters, coupled with a proportionate increase in legislative acquiescence in judicial determination. Morgan borrows his theoretical framework from views advocated in the formative years of the republic. He is concerned primarily with the "tripartite" and "judicial monopoly" theories. The former holds that Congress and the executive, as well as the judiciary, are obligated to consider the constitutionality of public policy. There are variations on this theme, ranging from Jefferson's emphasis upon the responsibilities of the political branches to Marshall's advocacy of juristic claims. Judicial monopoly, on the other hand, advances the idea of judicial pre-eminence in constitutional questions, and its spokesmen have appeared in all three branches of government.

This is a major work of constitutional history and of a real problem in current governmental practices. A brief review here neither can catalogue its many virtues nor come to grips with some serious defects. The main problem is that the author has taken congressional assertions of constitutional interpretation too literally, often viewing them as metaphysical abstractions, rather than as mere rationalizations either for or against some proposal of public policy. Few men in American political life (and especially congressmen) have had a consistent attitude toward the problem. Thus Jefferson favored judicial review in the 1790's and opposed it after he became President; William Howard Taft berated Congress in 1913 for "passing the buck" on prohibition to the Court, but eight years later as Chief Justice, of course, he insisted that the judiciary had primary responsibility for determining constitutionality; and present-day southern congressmen have insisted that *Brown v. Board of Education* determined nothing, while urging submission to the Civil Rights Cases (1883) and *Plessy v. Ferguson* (1896) as proper readings of the Fourteenth Amendment and therefore the "law of the land." Was Barry Goldwater's Senate speech, denouncing the 1964 Civil Rights Act as "unconstitutional," rooted in any conscious awareness of a tripartite theory of constitutional determination, or was he really responding to the push-pull pressures of a national constituency that possibly might give him the presidency?

There are many ways to approach this subject, and therefore Morgan's book can be questioned on numerous methodological points. But it must be emphasized that his work deserves the considered attention of every student of the political and legislative processes. It illuminates legislative behavior and the phenomenon of judicial power, in both their historical and current contexts. Morgan has, above all, affirmed the important proposition that the legal process and constitutional history involve more than courts and cases.

University of Wisconsin

STANLEY I. KUTLER

MONUMENTAL WASHINGTON: THE PLANNING AND DEVELOPMENT OF THE CAPITAL CENTER. By *John W. Reps*. (Princeton, N. J.: Princeton University Press. 1967. Pp. xv, 221. \$12.50.)

"George Santayana once wisely observed, 'Those who cannot remember the past are condemned to repeat it.'" Thus explaining his reasons for analyzing the character of L'Enfant's plan of the city of Washington and, in greater detail, the work of the Park Commission of 1901, John Reps states: "It is one of the purposes of this book to provide an understanding of the many attempts over the years to build a capital city worthy of a great nation and to reach some conclusion about their wisdom and effectiveness." A chapter describing the physical changes brought about in the more than a century-long interval between the 1901 plan, and a few pages outlining the new Pennsylvania Avenue plan for 1964 complete a review of 175 years of architectural and landscaping development in the central segment of the city's public domain.

An account of L'Enfant's training and experience illuminates the discussion of the Frenchman's design of 1791. In tracing the violence done to the plan in the nineteenth century, Reps repeats the popular myth that Andrew Jackson was responsible for placing the Treasury building where it broke the sweep of Pennsylvania Avenue from the White House to the Capitol. An economy-minded Congress fixed the site. This and one or two other factual errors are, however, minor flaws in a book of compelling interest. Magnificent illustrations clarify the text and throughout enhance the reader's pleasure.

The heart of this study deals with the proposals of the late 1890's and 1900 for redeeming the Mall and enlarging the White House, with the procedures and achievements of the Senate Park Commission of 1901, and with the ensuing political maneuvering necessary to carry out the essentials of the scheme. The story presents lessons not only in devising appropriate urban design but in persuading legislators to accept the results. Among the possible criticisms of the plan and its execution, one, Reps acknowledges, may be the stress on embellishment of the governmental core of the city at the expense of slum clearance and neighborhood improvements elsewhere. But, Reps points out, "only recently has the proper allocation of urban resources between capital improvements and social welfare become a matter of systematic study." In 1901 and for decades thereafter the focus of attention was on "the strategies and tactics of creating civic beauty." Like the men whose work it examines, this book confines itself to that topic.

Washington, D. C.

CONSTANCE McLAUGHLIN GREEN

GIDEON LINCECUM, 1793-1874: A BIOGRAPHY. By *Lois Wood Burkhalter*. (Austin: University of Texas Press. 1965. Pp. viii, 362. \$6.50.)

At first glance the reader fears that this book will be one of those life-and-letters biographies, a genre that quite fortunately has gone out of fashion. It consists of long quotations from letters and reminiscences tied together by passages of what is almost editorial prose. The impression may be true, but this is an exceptional case. Gideon Lincecum was an unclassifiable original who wrote

colorfully and well; Mrs. Burkhalter has chosen wisely to let him speak for himself.

While Lincecum was a man of many interests, his biography should be most useful to students of the southern frontier, whose child he was. Born in Georgia, reared there and in western South Carolina, he was eventually an Indian trader and physician in Mississippi, a farmer in Texas, and, for a time, a member of the Confederate refugee colony in Mexico. Quite apart from his wanderings, Lincecum has significance for his record of Choctaw myths and, in his later years, for his work in natural history. The latter may have been an outgrowth of his largely self-taught botanical medicine. He did correspond with various members of the Academy of Natural Sciences of Philadelphia, with Spencer F. Baird of the Smithsonian, and even with Charles Darwin, who read Lincecum's somewhat anthropomorphic account of "agricultural ants" to a skeptical Linnaean Society of London.

Burkhalter's treatment of Lincecum is sympathetic and competent. The topical arrangement of the volume does create a certain amount of repetition even as it clarifies the many aspects of Lincecum's career. My principal complaint is that footnotes are confined to identifications while citations are lumped together in "A Note on Sources" at the end of each chapter.

University of Delaware

GEORGE F. FRICK

THE FRONTIER RE-EXAMINED. Edited by *John Francis McDermott*. (Urbana: University of Illinois Press. 1967. Pp. vii, 192. \$6.95.)

JOHN Francis McDermott, masterful planner of conferences and editor of their by-products, launched this volume when he called together thirteen historians, ethnohistorians, students of literature, and archivists with an injunction to prepare papers that would not attack the views of Frederick Jackson Turner but "discuss aspects of the frontier not accounted for by him." This proved a broad assignment; Turner's own interest in the frontier dwindled so rapidly after his 1893 paper that his sparse writings on the subject left most of the field "not accounted for." The result is a collection of essays of varying degrees of excellence ranging from a discussion of Spain's reaction to the American exploration of Louisiana to the philosophy of the cowboy.

Most of the essays adhere to the editor's command not to attack Turner, although several waste valuable space demonstrating that he was guilty in his early essays of certain sins of omission or commission. Only one, Oscar O. Winther's suggestive interpretation of western transportation, seeks to determine Turner's actual knowledge of the subject by consulting his later writings and unpublished papers. The editor violates his own injunction by lustily beating Turner's bones in proving that by selecting the right spot (in this case St. Louis) a historian can prove that the frontier was an area of transplanted urban culture rather than of rural barbarism.

More significant is a common theme that appears in many of the essays. Western history, they insist, should turn from glamorous popularization to the basic studies that would link expansionism with the broader stream of American life. Thus Winther outlines research needed on the economics of

transportation, Herman R. Friis pleads for more accurate cartographical knowledge to correct distortions in the story of expansion, Ralph E. Morrow in a particularly challenging study fits western revivalism into the religious complex of its day, John C. Ewers pleads with authors to select contemporary illustrations for their accuracy instead of their color, Oliver W. Holmes urges investigation into the fundamentals of territorial government instead of vigilance committees and lawlessness, and Richard E. Oglesby presents a case study of St. Louis between 1820 and 1823 to prove the wide influence of the fur trade on the business structure of that day. This is western history at its best, and if historians of the frontier follow the advice of these authors, their discipline will gain both importance and respectability.

To single out the essays that illuminate this theme is to do injustice to others in the volume that are almost equally suggestive. Future scholars will certainly find inspiration in those that explore the "image" of the frontier; Donald Jackson views American explorers as seen by Spanish officials in Louisiana, Preston Holder looks at fur traders through the eyes of the Indians of the trans-Mississippi West, Jules Zanger and George R. Brooks examine the frontier image as captured by American and German fiction writers in the first half of the nineteenth century. Two other essays defy categorization; one by Merrill J. Mattes expertly describes the embarkation towns used by overland emigrants, and Joe B. Frantz gropes delightfully but unsuccessfully toward formulating a philosophy for the cowboy.

The volume is handsomely printed, well indexed, and enlivened by a number of maps and contemporary illustrations to illuminate the essays on cartography and art by Friis and Ewers.

Huntington Library

RAY ALLEN BILLINGTON

AMERICA'S WESTERN FRONTIERS: THE EXPLORATION AND SETTLEMENT OF THE TRANS-MISSISSIPPI WEST. By *John A. Hawgood*. (New York: Alfred A. Knopf. 1967. Pp. xxiii, 440, x. \$10.00.)

THE nineteenth-century drama of the conquest of the trans-Mississippi West is the theme of *America's Western Frontiers* by British historian John A. Hawgood. Awarded the Alfred A. Knopf Western History Prize, given biennially for a manuscript "written with distinction and in which sound scholarship may be taken for granted," Hawgood's study is drawn from both primary and secondary materials, as well as from a personal acquaintance with the West gained from extended trips to the United States. Conjuring up with a masterful pen the principal personalities and events of the Old West, the author produces a vivid, engaging story that the general reader will find fascinating and the historian may read with profit.

In his opening chapters Hawgood speculates on the evidences of early man in North America, then surveys in some detail the Spanish intrusion from the south. Hopping east of the Mississippi, he follows Lewis and Clark west, glances at Pike and Long, and revels with mountain men. In the story of westward migration, the settlement and independence of Texas pale in the light of the Mormon experiment. At mid-point Hawgood describes the gold rush, then

traces the mining frontier inland. Water and land travel are discussed next. The writing again becomes vivid with the decline and fall of the Indian barrier. Well-drawn portraits of famous Plains chieftains appear, but the handling of the Apache problem is inadequate, especially the Camp Grant Massacre. Ramon Adams enlivens the chapter on the cattle frontier by his unvarnished description of the old-time cowboy. The influx of the farmer marks the end of the traditional story, but Hawgood is quick to point to other frontiers yet existing in the West, citing the urban frontier and the great underdeveloped Alaskan country.

Hawgood's book is aimed at the general reader. Because this work inevitably will be on reading lists for college history courses, however, a word of caution is in order. In the first place, overgeneralizations abound. Archaeologists would question the claim, for example, that there was little exchange among southwestern cultures in the pre-Columbian period, pointing to Mexican trade goods found in Arizona ruins. Historians probably would have second thoughts about calling the Chivington Massacre America's St. Bartholomew, and they would bemoan the omission of such well-known episodes as the Lincoln County and Johnson County wars. There is confusion, furthermore, over geography in the text and the maps. Finally, the habit of breaking into the narrative to make personal observations and to discuss other historians is often disconcerting.

The book carries numerous illustrations and includes articles from *Hutchings California Magazine* on the gold rush and the pony express. An eighteen-page bibliographical essay suggests further reading. The index is inadequate, and the proofreader grew tired beginning with Chapter VIII. Whatever its shortcomings, *America's Western Frontiers* is a beautiful book both in design and format. As a popular introduction to the Old West, it stands without peer.

University of Arizona

HARWOOD P. HINTON

DANIEL WEBSTER & THE SUPREME COURT. By *Maurice G. Baxter*. ([Amherst:] University of Massachusetts Press. 1966. Pp. ix, 265. \$6.75.)

THIS is a thoroughly researched, well-written study of Webster's arguments before the Supreme Court. After describing his development as a lawyer, Baxter devotes a chapter each to Webster's briefs in cases concerning Supreme Court jurisdiction, the contract clause, the rights and uses of property, and the commerce clause. There are additional chapters on the Dartmouth College case, the national bank, and legislative proposals for judicial reform.

Baxter's thesis is that Webster was "the chief crystallizing agent, so to speak, in many of the most important decisions" of the Supreme Court that shaped the meaning of the Constitution from 1815 to 1850. One cannot quarrel with this conclusion, but one can question its significance. In a sense it expresses a truism, for it is precisely the task of lawyers to place before the justices interpretations of the Constitution from which to choose. Studies of lawyers' arguments such as those by Arnold Paul or Clyde Jacobs are illuminating because they show how new constitutional understandings on certain key issues emerged from a variety of legal sources at the same time. By confining his attention to one lawyer's arguments on many issues Baxter has produced a work that is

valuable for students of Webster, but less so for students of constitutional history. For the latter the book amounts to a review of many cases of the Marshall and Taney courts with emphasis on Webster's part in them. Baxter's thesis, moreover, is not new, but may be found in Everett P. Wheeler's *Daniel Webster: The Expounder of the Constitution* (1905). Baxter's work is more thorough and critical, but in essence does not go beyond Wheeler's conclusion that the arguments of Webster were as necessary as the decisions of Marshall in the development of American constitutional law.

A problem Baxter touches on, and might have examined further, is the determination of constitutionality. The author sees Webster as a judicial supremacist who assigned little responsibility to the legislative and executive branches in interpreting the Constitution. In July 1832, however, Webster explained that acts of Congress could "settle the construction of the Constitution. . . ." Should a law be challenged, its constitutionality might be decided by the courts, but otherwise the legislative action, invariably preceded by exhaustive interpretation of the Constitution, stood as the determination of constitutionality. Had Baxter taken this fact into consideration, he might have found Webster the congressman not a simple judicial supremacist, and he would have contributed to our understanding of an important problem in constitutional history.

University of Maryland

HERMAN BELZ

THE LAW AT HARVARD: A HISTORY OF IDEAS AND MEN, 1817-1967. By *Arthur E. Sutherland*. (Cambridge, Mass.: Belknap Press of Harvard University Press. 1967. Pp. xv, 408. \$9.50.)

ARTHUR E. Sutherland has written an absorbing account of America's leading law school on the occasion of its 150th birthday. Isaac Parker, then also the Massachusetts chief justice, was elected in 1815 the first Royall Professor of Law, thus becoming Harvard's first law professor. In 1817 the Overseers established a school of law, with a faculty of two. In the early days, the school was small and struggled to survive; in 1829 it had only two students. But in June 1829 Justice Joseph Story accepted appointment as the first Dane Professor, and while retaining his seat on the Court, he served as a professor for many years, teaching students with great distinction and publishing an astonishing number of learned legal treatises. From then on the health of the Law School was never in doubt.

Sutherland's history of the school is also a contribution to the history of legal education in the United States, for, at least until recent years, it was a leading innovator. The author concentrates upon four main ingredients of the school: its students, its faculty, its library, and its buildings. A distinguished school needs both great students and great professors, and on this subject Sutherland has no doubts at all. He manages, in one way or another, to say something about every member of the Harvard Law School faculty since its birth in 1817, and about many of its very eminent graduates.

Most of the story of the school is organized around its deans, beginning with the first, Christopher Columbus Langdell, who was appointed in 1870 by President Charles William Eliot. Langdell was the true architect of the Law School. He imposed admission requirements and examinations, lengthened the course

from eighteen months to three years, and introduced the case system of instruction. All of the school's buildings are described, and the growth of the library is recounted in each chapter in considerable detail. The author discusses the founding of the Law School Association (1886) and the *Law Review* (1887), enrollment figures from year to year, fund raising and other financial problems, and many other aspects of a thriving educational institution.

While the penultimate chapter deals with the deanship of Erwin N. Griswold, who was chosen in 1946, Sutherland understandably says little about the career of one who is still the school's chief administrator. He describes instead some of the emerging problems and tendencies of the school, such as the size and space problems, the shift from the preoccupation with the analysis of judicial opinions to the study of statutes, the increasing involvement of the faculty in extra-curricular, and particularly governmental affairs, the drift toward greater emphasis upon self-education for the students, the impact of an expanding state welfarism upon legal education, and the growing emphasis upon international and comparative legal studies.

No doubt Sutherland has made a solid contribution to American legal history, but this is still the book of an active and worshipful member of the present faculty whose monumental assumptions about greatness require critical analysis. A broader history of legal education will someday put the story of the Harvard Law School in a more objective framework. It will take into account the contributions of other law schools and of other professors and students of the law, and it will not be written by an alumnus or by a faculty member of the school.

University of Wisconsin

DAVID FELLMAN

GEORGE TICKNOR AND THE BOSTON BRAHMINS. By *David B. Tyack*. (Cambridge, Mass.: Harvard University Press. 1967. Pp. x, 289. \$6.95.)

GEORGE Ticknor, professor of modern languages at Harvard (1819-1835) and author of the monumental *History of Spanish Literature* (1849), was himself something of a monument in mid-nineteenth-century Boston. Foreign sophisticates could better afford to miss a visit to Faneuil Hall than to miss a chat with the lion of Park Street. Yet Ticknor, according to David Tyack, has shared the fate of the many patrician worthies "eminent in their day and forgotten in ours."

Ticknor has scarcely been forgotten. He has been lauded by Stanley T. Williams as "the father of our Spanish scholars," by Samuel E. Morison as a prophet of academic reform, and by Van Wyck Brooks and others as a cultural ambassador important symbolically and actually in the flowering of New England culture. But Tyack's contribution is a valuable one; it is an examination of Ticknor's relation to nineteenth-century aristocratic leadership.

The author's point of view, almost a mirror image of Martin Green's in his recent *The Problem of Boston*, is a kindly but firm disapproval of Brahmin conservatism. While Green's Ticknor is the serenely self-fulfilled leader of a saving remnant, Tyack's is the high priest of a doomed cultus—a man who not only lived through but lived out the anguished dilemmas of a "tory republicanism." Behind the brave public masks of Ticknorville (Nathan Hale's name for Bos-

ton) and its distinguished minister of culture, Tyack detects the private despair of men who knew they had been born too late.

This interpretation shows strain when Tyack tries to construe Ticknor's habitual good humor as dutiful rather than heartfelt, or when he reads "grim transformations" into the contrasts between a Sully portrait of 1828 and later portraits by less saccharine artists. But Tyack's view, documented from primary materials that Green, for example, does not claim to have examined, is clearly entitled to respect.

The theme and point of view are presented discursively, rather than being deeply probed or closely reasoned. The book opens with an evocative chapter on the Boston and New England of Ticknor's youth, and then, in five successive divisions, recounts his foreign wanderings, his teaching career, his views of national literatures, his leadership in Boston society, and his growing distress about the course of American public life. Though this is certainly not "Ticknor day by day" biography, the author's design does not enable him to pause for long even at those junctures that would seem to be critical in any intensive exploration of the announced subject. Tyack can spare only seven pages, for example, for a direct discussion of that *History* which secured its compiler's claims as *doyen* of Brahmin culture. Only three pages are available for Ticknor's epochal achievements as "the moving spirit behind the Boston Public Library."

Tyack is at his best when he settles into a more ample treatment of Ticknor's third great project: his reforms and attempted reforms at Harvard. If it would make room for more such discussions, one could forego retracing the tedious path of young Ticknor's international base touching—a subject for analytic seven-page treatment if ever there was one.

American University

WILLIAM R. HUTCHISON

EWING YOUNG, MASTER TRAPPER. By *Kenneth L. Holmes*. (Portland, Ore.: Binford and Mort for the Peter Binford Foundation. 1967. Pp. viii, 180. \$5.95.)

EWING Young was a cabinetmaker's apprentice from Tennessee who accompanied the first wagon train from Missouri to Santa Fe in 1822 and, during the next decade, trapped beaver between the Pecos and the San Joaquin. Later he traded stock in the Southwest, hunted sea otter in California, drove the first large herd of cattle from San Jose north to the Willamette, and accumulated a baronial domain in the Chehalem Valley. To probate his valuable estate—he died in 1841—his neighbors created the first American squatter government in Oregon.

Although Young was literate and active the records of his life are sparse and difficult to locate. Professor Kenneth L. Holmes of Linfield College, supported by a research grant from the Peter Binford Foundation of Portland, tracked Young through the archives and followed his trails from Missouri to the West Coast. Holmes's carefully researched study elaborates upon Joseph J. Hill's article on Young in the Southwest, carried in the *Quarterly of the Oregon Historical Society* in 1923, and compares favorably with Harvey L. Carter's biographical

essay on Young published in LeRoy Hafen's *The Mountain Men and the Fur Trade of the Far West*, Volume II (1965).

Young's early life in Tennessee and Missouri is difficult to document. Holmes relied upon correspondents in the two states to dig out county probate and deed records for him. One wonders if his correspondents examined county court records and searched for tax manuscripts that might illuminate the Young family. Holmes notes that the "wording of the will" of Young's father "indicates that the Youngs were of one of the frontier evangelical Sects," but offers no explanation of the premise. Another of his statements, that Young showed "peculiar genius" in transforming himself "from a trapper to a settler," must be qualified since several fur trappers and traders made the transformation successfully.

The book is profusely illustrated and adequately footnoted, yet lacks a formal bibliography. Reviewers must examine each footnote minutely to compile a picture of the author's research. But in spite of minor problems of citation, methodology, and interpretation, Holmes's monograph is a praiseworthy scholarly biography.

University of Texas, Austin

JOHN E. SUNDER

POLICING THE CITY: BOSTON 1822-1885. By *Roger Lane*. [Publication of the Center for the Study of the History of Liberty in America, Harvard University.] (Cambridge, Mass.: Harvard University Press. 1967. Pp. x, 299. \$8.50.)

HISTORIANS talk much about the transition between *Gemeinschaft* and *Gesellschaft*. Most of the talk depends heavily upon a few classic, generalized schemata that are treated as if they were themselves empirical evidence.

Roger Lane's *Policing the City* breaks out of this conceptual rut with a series of new insights and supportive evidence. Lane shows that formal codes were frequently developed to protect traditional notions of justice against innovative but autocratic attempts to control the new forces of social dissension. The elaboration of these codes and of administrative procedures required major changes in the locus of both public and private decision making. At the beginning of the nineteenth century the Boston police simply came to the aid of private citizens. Only in slow steps did the police articulate goals separate from those of private individuals and pre-empt the functions of control to themselves. Some of the best sections of Lane's book, treating theft and murder, clarify these steps and their administrative logic. A private individual who has been robbed is principally interested in recovering his funds; the public is more vitally concerned with apprehension of the thief. Until the police developed a structure of rewards and supervision to fulfill the public goals, they operated as agents of private parties. Comparably, much administrative growth was necessary before the police could assume the responsibility of apprehending murderers. This growth depended in many ways upon the police department's own specialization in a few areas of social control. At the beginning of the century the police were an omnibus agency. By 1885 they had given up—sometimes gratefully, sometimes reluctantly—many licensing and welfare functions to other departments.

Lane chooses 1885 as the terminus of his book, arguing that by that time the

Boston police had achieved an administrative equilibrium that persisted even under a new form of state direction. The deepest basis of this equilibrium was general acceptance of the forms that the police power had taken. "The police department," he writes, "was in fact what it was designed to be, simply a useful tool of government. So long as government itself was still regarded as a tool of the people, the citizens remained confident of their ultimate ability to use both as desired."

Lane would probably agree that the Boston equilibrium was not, and is not, universal in every American city.

University of Pennsylvania

SEYMOUR J. MANDELBAUM

THE GALENA LEAD DISTRICT: FEDERAL POLICY AND PRACTICE, 1824-1847. By *James E. Wright*. (Madison: State Historical Society of Wisconsin for the Department of History, University of Wisconsin. 1966. Pp. xv, 148. \$3.25.)

THIS short, well-written volume provides an interesting addition to the literature on public land policy between 1807 and 1847. Previous studies have given only perfunctory treatment to the application of the federal leasing system of the lead ore lands, the subject of this study.

In 1807 Congress passed legislation to reserve lead lands and to provide for a leasing system of those lands. For the payment of 10 per cent of the lead ore mined, a miner could obtain a lease and work a small site of the public domain. The law was first applied to the Missouri lead mines, but the land situation in that region was too complex for effective enforcement. In the early 1820's federal officials turned their attention to the upper Mississippi Valley. Present-day Jo Daviess County in Illinois and Lafayette, Iowa, and Grant Counties in Wisconsin, the effective boundaries of the public mines, had not been previously settled, and the Ordnance Bureau of the War Department, charged with the administration of the 1807 act in 1821, was able to inaugurate the federal leasing system contemporaneously with settlement.

Under the capable superintendency of Lieutenant Martin Thomas, from 1824 to 1829, the leasing policy was successfully administered. Miners who had rejected leasing in Missouri accepted it in the Galena district. Wright attributes this initial success to an abundance of ore, a lack of an established system of mining, and the absence of conflicting private landownership.

Little opposition existed to the system prior to 1829. In that year, however, Thomas was removed, apparently under pressure from Senator Benton. This factor, combined with falling lead prices and soaring food costs, led to agitation among the miners and government-licensed smelters. Thomas' successor, Captain Thomas Legate, was an inept administrator and, in addition, was highly critical of the leasing system. By 1836 the federal leasing policy existed only in law.

Attempts to restore the system in the 1840's failed, and by 1845 the public mines were no longer operated. Shortly afterward Congress empowered the President to desocialize the mineral reserve lands.

The Galena Lead District is well researched and based on a wide range of

sources. The student of mining history, as well as of land policy, will find this local history study well worth the reading.

University of Texas, Austin

L. TUFFLY ELLIS

THE 1826 JOURNAL OF JOHN JAMES AUDUBON. Transcribed with an introduction and notes by *Alice Ford* from the original in the collection of Henry Bradley Martin. (Norman: University of Oklahoma Press. 1967. Pp. xii, 409. \$6.95.)

THIS journal, which takes Audubon from Louisiana to Liverpool, Manchester, and Edinburgh, covers a particularly critical period in his career, his effort to publish *Birds of America*. It is also useful in giving insight into its author's complex character. He arrived in Liverpool long haired, an exotic of the American forests, a bit of showmanship that surely did not decrease his meager earnings from displaying his drawings. Showman that he was, though, he faced British society with the same insecurity that had caused him so often to fabricate his past.

Miss Ford is the author or editor of four earlier books on Audubon, including a detailed biography that effectively used this journal. She comes to her task as editor with affection and deep knowledge. Her transcription of the manuscript has utilized common sense in expanding abbreviations and in supplying materials to make the journal readable without resorting to the bowdlerization of the earlier version by Maria Rebecca Audubon. The editor's efforts are eased by the fact that Audubon wrote well even things that he did not intend for publication. The footnotes in this volume supply needful identifications of both major and minor figures who crossed Audubon's path. She errs oddly, however, in confusing George Canning with William Ellery Channing and in suggesting that Audubon crossed the equator while still in the Gulf of Mexico. Much more important for students of Audubon, Ford again makes the point that her subject could not have studied with Jacques Louis David, although her earlier biography should have laid that myth to rest.

The volume is a useful addition to literature on Audubon. The University of Oklahoma Press has, moreover, given it a handsome format and good black-and-white illustrations of sketches from the original journal and other drawings and paintings by Audubon from his British stay. It should be useful to the specialist and appeal also to the collector of reasonably priced illustrated books.

University of Delaware

GEORGE F. FRICK

ANDREW BROWN AND CYPRESS LUMBERING IN THE OLD SOUTH-WEST. By *John Hebron Moore*. ([Baton Rouge:] Louisiana State University Press. 1967. Pp. xv, 180. \$6.00.)

JOHN Hebron Moore, author of a thoughtful history of *Agriculture in Ante-Bellum Mississippi*, has now produced a thoroughly researched, well-organized, and significant study of a medium-sized lumber firm of Natchez and New Orleans. Andrew Brown began sawing cypress logs at Natchez in the 1820's and over the course of the next forty years built up a substantial business cutting

logs, mostly on the public lands in the Yazoo Delta, floating them down to Natchez where they were sawed into boards for local sale and later for the New Orleans market. The papers of Brown and his firm, which recently came to light, have enabled the author to trace in detail the acquisition of the logs, the gradual improvement of the mill at Natchez with the introduction of new and powerful steam engines, gang saws, and other equipment, the sale of dimension lumber, and the establishment of a yard in New Orleans that took an increasing amount of the boards from the mill. The hazards, profits, and losses of the business are explained in some detail.

Although the manuscript records of the Brown firm provided much of the basic story, Moore was not content to rely solely on them, but worked through the newspapers of Mobile, New Orleans, and a number of Mississippi communities as well. He has been able to fit his story into the general context of the economic growth of the Deep South and particularly of its lumber industry. His concern for the details of logging, rafting, milling, shipping, and marketing, which are always nicely fitted into the account, makes this one of the best histories of the lumber industry. Moore gives considerable attention to the use of slave labor and the confidence Brown had in some of his skillful hands in the operations of the mill and in purchasing logs, soliciting orders, shipping and selling the finished product.

The study is admirably planned and executed, and Louisiana State University Press has brought it out in excellent form with the footnotes at the bottom of the page where they should always be in scholarly publications.

Cornell University

PAUL W. GATES

WORKSHOPS IN THE WILDERNESS: THE EUROPEAN RESPONSE TO AMERICAN INDUSTRIALIZATION, 1830-1860. By *Marvin Fisher*. (New York: Oxford University Press. 1967. Pp. viii, 238. \$6.00.)

In this book a sensitive student of American literary and cultural values seeks to use the abundant travel literature of ante bellum America for two main purposes. One is to reveal differences between European and American methods of manufacturing and to supplement statistical and other sources of information concerning the extent and rate of industrial expansion. The other is to show the impact of technology on the preindustrial values of Americans in art, design, and architecture. His basic assumption is that perceptive foreigners were better able than most Americans to "isolate and identify what the culture-bound native observer might overlook."

The disclosure of national differences is an objective well served by some of the witnesses called upon by Professor Fisher. In general, however, the lack of precision and comparability makes it difficult to sum the testimony of impressionistic observations into a reliable view of either the extent or rate of industrial growth. Fisher equates "scattered" comments on industrialization in the 1830's, "more frequent" ones in the 1840's, and a "torrent" of them in the 1850's with the pace of growth itself, which he is convinced occurred with relative suddenness and rapidity between 1830 and 1860. It is more likely the process was one of longer duration. Robert W. Fogel has recently assembled evidence of striking

industrial advance in the 1820's, and it is probable the date can be extended back to 1807, at least. In sum, data yielded by state censuses and national reports, together with studies of individual industries by scholars, provide a sounder basis for generalization than do travel accounts. It is true that those accounts find support, as Fisher notes, in W. W. Rostow's suggestion that a take-off occurred between 1843 and 1860. But that suggestion is now thoroughly discredited, most notably by the criticism of Simon Kuznets. As Fogel well observes, we should not require the Industrial Revolution to have the swiftness of a *coup d'état*.

Fisher achieves a higher degree of success in his second objective. His chapter on "The Iconology of Industrialism" is a subtle analysis of ambivalent European commentary that shows how an older set of agrarian values was stretched to accommodate the promise of the machine. At the same time, this ambivalence, he suggests, impeded a direct expression of function in form and "greatly hindered the diffusion of functionalism as a principle in architecture or industrial design."

Columbia University

STUART BRUCHEY

THE DEATH OF SLAVERY: THE UNITED STATES, 1837-65. By *Elbert B. Smith*. [The Chicago History of American Civilization.] (Chicago: University of Chicago Press. 1967. Pp. viii, 225. \$5.00.)

THIS volume, one of a series of twenty-four arranged in two groups, one chronological and the other topical, is designed to analyze the character and the evolution of the culture of the United States by presenting in compact and readable form the latest historical insights of American scholars.

Elbert B. Smith, the author of a well-regarded biography of Thomas Hart Benton, who was one of the significant figures in the politics of this era, is admirably prepared to deal with the internal strife that is the dominant theme of this volume in the chronological group. He deals with a period of cold war characterized by certain "man made or at least man affected phenomena" projected by racism in a fashion resembling the present period of social and political tension. These phenomena, he believes, "might have been different," and he hopes they "need not be repeated."

The completion of his task required the author to compress his account of the most complex quarter century of the nation's history within two hundred pages. He does this effectively within the frame of reference supplied by the symbolism contained in the title. In tracing the destruction of the institution of slavery he is primarily concerned with how the republic dealt with an institution so contrary to its spirit. The existence of this system of forced labor in such a polity and in such an ecology almost inevitably must produce a social conflict, and, in a democracy, social conflict will nearly always be put to political use in the periodic struggles for power. This conflict for the power to destroy the institution is the main theme of the book.

The historian's task is complicated by the fact that the period was such a romantic age in which the prevailing atmosphere of unreality constantly misleads. The strength of this work is the author's capacity to penetrate the mists of unreality, revealing many glimpses of truth. He prepared himself for this

not only by studying Benton, one of the most spectacular of the romantics, but by participating in today's politics as a sort of moonlighting phase of his academic absorption, thus learning something of the truth about the actual "choices" that men "face" in the practice of government.

Smith makes effective use of his chosen symbolism and of his perceptive grasp of some of the implications of this romantic age in which realities were often meaningless. For he has discovered how false were many of the premises, how groundless the fears, and how faulty the logic of even such great leaders as Lincoln, Calhoun, Davis, and Douglas as they misapprehended the situations they faced and fell victims of the prevailing confusions of their era. This was exaggerated by their efforts to fit ideas and institutions developed in an age of reason into the frame of reference of the romantic age that succeeded it. In an era of a misunderstood racism the American people went to war conscious perhaps, as Whitman suspected, that they would never see with their "own eyes the peerless power and splendid éclat of the democratic principle," but determined that they would never desert, despair, nor abandon their faith in it.

Destruction of the institution was complicated by a confusing variety of factors that no one has ever completely identified or tabulated. The unbelievable confusion caused a bloody and costly civil war that no one has yet been able completely to understand. Scholars are constantly lured into trying, and this is a significant attempt. It is not, nor is it designed to be, a comprehensive analysis; two hundred pages could not contain one. It is, however, something better for it sends the reader into the future with a thoughtful urge, a determination to command more thoughtful comprehension. The basic problems of race have not yet been solved, nor is their potential threat much less than it was in 1861. The problems of a century ago constantly assume a new significance that requires a new dialogue and not simply a repetition of the clichés of a romantic age.

University of Pennsylvania

ROY F. NICHOLS

WESTERN AMERICA IN 1846-1847: THE ORIGINAL TRAVEL DIARY OF LIEUTENANT J. W. ABERT, WHO MAPPED NEW MEXICO FOR THE UNITED STATES ARMY. Edited by *John Galvin*. ([San Francisco:] John Howell—Books. 1966. Pp. 116. \$7.50.)

THE Army of the West commissioned twenty-five-year-old Lieutenant James W. Abert of the Topographical Engineers, only four years out of West Point, to continue the first United States survey of New Mexico. As this young officer and his party traveled from Fort Leavenworth to the region south of Santa Fe, he delighted in portraying flowers, birds, elk, buffaloes, grizzly bears, antelope, and Indians and Mexicans. He not only collected minerals and plants; he actually skinned birds and prairie dogs.

Abert supplemented his notes with sketches and watercolors, but his short descriptions vividly convey his impressions and emotions. One can almost smell the piñon, feel the mules straining to cross rushing streams, and see the flat adobe dwellings and the lance-like yuccas rising against the prairie horizon. The reader shares his excitement over the flights of the many beautifully plumed

birds. It is easy to share his disgust when several of his men disobey his orders, and his indignation at the slovenly volunteer teamsters he encountered at Bent's Fort.

The diary provides insight into army life of the period, which, as today, was rife with rumors. Abert met with General Kearny, the Bents, Colonel Doniphan, Colonel Price, and the St. Vrain, and, after his return to Santa Fe, he shared a cup of chocolate with the famous Doña Julia. His contacts with these notables were brief, however, and he adds little to our knowledge of them.

The return journey during January and February almost proved disastrous. The party suffered from ague, dysentery, and measles, and endured blizzards, while worrying over a possible attack by the Arapaho. One man was kicked by a mule; another was blinded by snow; another froze to death in a snow storm. The mules were stolen by the Pawnee while the party was still over three hundred miles from civilization. Abert himself was discouraged.

The book contributes little about New Mexico's society during this period, but it splendidly describes the country. Perhaps as important, the diary reveals Abert as a sensitive, courageous, able young officer, carrying out his responsibilities despite formidable odds. It gives the reader a feeling of pride that he represented the United States.

John Galvin has done a fine job of editing. His notes clarify, but they do not intrude. He explains the correct spellings of names and events and includes a helpful glossary, mainly of Spanish terms. He has added an index, a bibliography, and two fine maps so that the journey can be traced in detail. The publisher also should be congratulated on a beautiful format, which includes a number of Abert's fine watercolors.

University of Texas, Austin

JIM B. PEARSON

GOLD RUSH DIARY: BEING THE JOURNAL OF ELISHA DOUGLASS PERKINS ON THE OVERLAND TRAIL IN THE SPRING AND SUMMER OF 1849. Edited by *Thomas D. Clark*. (Lexington: University of Kentucky Press. 1967. Pp. xxv, 206. \$8.75.)

WITH nearly two hundred gold rush diaries already in print, including such outstanding accounts as those by J. Goldsborough Bruff, Madison B. Moorman, and James A. Pritchard, another firsthand narrative of the overland migration to the California mines may well seem unnecessary. Yet Elisha Douglass Perkins' journal is distinctive enough to justify publication. Perkins was one of six young men calling themselves "the Marietta Gold Hunters," who set out for California from St. Joseph, Missouri, on May 29, 1849. The six abandoned their two wagons and split into three pairs at Fort Laramie, continuing on their own with three mules per couple. One of each of the three pairs died in California, two shortly after their arrival in the mines and Perkins late in 1852. None found enough gold to mention.

Perkins was an indefatigable recorder of his experiences and impressions. From the time he left St. Joseph until he reached Sacramento 121 days later he never missed a day, and most of his entries are extensive. Although Perkins and his partner took much longer than many of their contemporaries to make the

westward crossing and there were times when the going was exceedingly rough, a persistent air of optimism pervades the diary. Perkins commented upon the neighborliness and generosity of fellow emigrants, and, although he envied the relative comfort that ox-drawn wagon travel could provide, he recognized the disadvantages of the slower pace and the likelihood of breakdown on the road and concluded that the only way to cross the Plains and the mountains was with pack mules and ponies. His description of a buffalo chase and his account of bending a rifle barrel over the head of an unruly mule are particularly vivid.

Editor Thomas Clark enhances the value and interest of Perkins' diary by numerous and extended footnotes quoting others who passed along the same way either before or after Perkins. These parallel commentaries from such travelers as John C. Frémont, Edwin Bryant, J. Goldsborough Bruff, and about twenty others provide a useful extra dimension to the book. On the other hand, Clark's introduction, except for the background it supplies on Perkins and his companions, is largely superfluous, sometimes misleading, and not as well written as the journal he edits.

University of Oregon

EDWIN R. BINGHAM

NEWSPAPERS ON THE MINNESOTA FRONTIER, 1849-1860. By *George S. Hage*. ([St. Paul:] Minnesota Historical Society. 1967. Pp. ix, 176. \$4.50.)

THIS book, written by a professor of journalism at the University of Minnesota, competently describes the first decade of Minnesota journalism. A fifteen-page epilogue establishes the connection between the main part of Professor Hage's story and Minnesota's leading newspapers of the present day, the *Minneapolis Tribune* and the *St. Paul Pioneer-Press*.

Although it has sometimes been said that newspaper editors were in the vanguard of the westward movement, this was not strictly true, at least for Minnesota. Almost thirty years after the first American settlers occupied land claims near Fort Snelling, ten miles south of present-day Minneapolis, James M. Goodhue published the first number of the *St. Paul Minnesota Pioneer*, the first newspaper established in Minnesota Territory.

The history of Minnesota journalism in the pioneer stage was not greatly different from that of other western states and territories. The first Minnesota newspapers were weeklies, and when, in 1854, St. Paul grew to four thousand, it acquired four daily newspapers almost simultaneously, three years before Minneapolis started its first daily. Early Minnesota newspapers vigorously promoted settlement, brought news of the outside world to their readers, mirrored the more agreeable features of frontier life, and engaged in party journalism. Their most obvious shortcoming was their tendency to ignore local news and national issues other than slavery.

Among the "firsts" that receive attention in this volume are the first newspaper in the Northwest "and perhaps in the country" to carry the name of the newly organized Republican party, the first convention of Minnesota editors and publishers, the first formal interview (with a condemned murderess) to appear in a Minnesota newspaper, and the first sports event to receive extensive coverage in the Minnesota press. Perhaps the most significant event treated in the

book is the heartening story of the successful fight of crusading editor Jane Grey Swisshelm of the St. Cloud *Visiter* in behalf of freedom of the press.

After the Civil War, as Hage points out, the Minnesota press developed along lines characteristic of the American press as a whole. Well illustrated and containing a useful appendix listing the Minnesota newspapers of the 1850's chronologically, this little volume reflects credit upon the Minnesota Historical Society, which collected the materials upon which the book is largely based.

Chatham College

J. CUTLER ANDREWS

STAGECOACH WEST. By *Ralph Moody*. (New York: Thomas Y. Crowell Company. 1967. Pp. x, 341. \$6.95.)

IN broad outline this study of stagecoach operations in the trans-Mississippi West describes how Sacramento, California, became the stagecoach center of the world in the 1850's as a result of gold discovery. After briefly noting that stages ran on the Santa Fe Trail, the author turns his attention to the evolution of overland mail and stagecoach services—the main theme of his book. Starting with the pioneer enterprises attempting to bridge the gap between the Missouri River and the Pacific slope, he tells his story, using the promotion and organization of business partnerships and companies as the frame of reference. First there was John Butterfield and his overland “ox-bow” route through the Southwest, followed by the decision of the freighting firm of Russell, Majors and Waddell to enter the stagecoach business into Colorado and along the central route as a competitor. Wells, Fargo & Co. assumed control of the Overland Mail Company when it moved to the central route; Ben Holladay assimilated Russell, Majors and Waddell and then sold out to Wells, Fargo & Co. in 1866 when the latter company established a near monopoly of stagecoaching west of the Missouri River. This company's control was brief because of the completion of the first transcontinental railroad, and thereafter the stagecoaches served as feeders to the railroad. To enliven this narrative of business histories, the power struggle among the moguls of the express and mail enterprises and their machinations with the government to obtain subsidies, the escapades of Black Bart and Jack Slade, and the careers of jehus like Charlie Parkhurst are vividly described.

Specialists in western American history will immediately conclude that this is an oft-told tale. Quite true, but it has never before been told in a single volume with such thoroughness and balance. Ralph Moody has mastered the pertinent secondary works, utilized published memoirs of participants with scholarly restraint and good judgment, and effectively quoted several new and unusually pertinent manuscript sources. He does not eschew interpretation nor hesitate to pass judgments.

Moody accepts my recently documented findings that Wells, Fargo & Co. assumed control of the Overland Mail Company in 1861 and thereafter operated it as a subsidiary. The company thereby controlled the pony express for the last third of its existence. Wells, Fargo also purchased the Pioneer Stage Line in 1864 whereby stages could be run into California by the company from Salt Lake. Moody is, however, confused as to the nature and relevance of the new

record books that have resolved a long-standing historical controversy concerning Wells, Fargo & Co. He continues to suggest that John Butterfield resigned as president of the Overland Mail Company, whereas it is now clear that he was voted out of that position by the Wells, Fargo directors on the board.

An early, outstanding chapter of the book reveals that the author is thoroughly familiar with the construction and utilization of a stagecoach and understands the skill and importance of handling the mules and horses that drew it. He also expertly delineates the various stage routes in relationship to the present highway system. The maps are helpful, the illustrations well chosen. This is the best single-volume history of the frontier express and stagecoach lines that has appeared.

University of California, Davis

W. TURRENTINE JACKSON

OWEN LOVEJOY: ABOLITIONIST IN CONGRESS. By *Edward Magdol*. (New Brunswick, N. J.: Rutgers University Press. 1967. Pp. xi, 493. \$10.00.)

THE author has correctly characterized this book as "frankly partisan," a biography that reveals more of its subject's public than his private life. Much of it is a chronicle of the events of his life. Except for crediting his acts to a wholly selfless humanitarianism, the author makes little attempt to examine the factors that may have motivated the reformer-politician.

Owen Lovejoy, younger brother of Elijah Lovejoy, the martyred antislavery editor, settled in Princeton, Illinois. He served for seventeen years as a Congregational minister, helped organize antislavery activities, entered the Illinois legislature in 1854, and held a seat in Congress from 1856 until his death in 1864. As an early convert to political antislavery, Lovejoy participated in virtually every phase of that movement. He helped organize the Liberty party in Illinois and attended its national convention in 1843, the national convention of the Free Soil party in 1848, and that of the Democratic party in 1852; he became a leading figure in the founding of the Republican party in Illinois.

In Congress Lovejoy spoke vigorously against the Fugitive Slave Law. Although he supported a number of successful wartime measures, he was the sole author of only one—the law abolishing slavery in the territories. Lovejoy was primarily an orator, an agitator rather than a legislator.

The author's diligent research has led him to some manuscript and much more newspaper material. Some of the anecdotes included are, however, based on reminiscent accounts that were written many years after the events occurred. The treatment of the Underground Railroad and Lovejoy's participation in it accepts the traditional accounts. The differences between Lincoln and the Radicals are minimized, and the warm relationship between the abolitionist congressmen and the more moderate President emphasized. The author contends that the common objectives of preserving the Union and ending slavery overshadowed their differences. This is a well-researched, readable, and highly sympathetic account of Lovejoy's career.

Wilmington College

LARRY GARA

LINCOLN VS DOUGLAS: THE GREAT DEBATES CAMPAIGN. By *Richard Allen Heckman*. (Washington, D. C.: Public Affairs Press. 1967. Pp. v, 192. \$5.00.)

AN author must be brave indeed to venture another book on the Lincoln-Douglas debates. Richard Heckman admits in his preface that his subject has attracted both scholars and laymen for "over a century," but he hopes to review "with greater depth the background, issues, and significance of the entire campaign." He particularly regrets that lay readers have been trapped into thinking of the debates as "one of the greatest pivotal movements in history." Also he feels that the Freeport debate "has received far more attention than the facts would indicate it deserves." However, he provides no argument to support these statements, and the reader closes the book believing that the debates, especially the one at Freeport, may have been pivotal in American, and consequently in world, history.

Since Paul M. Angle edited a complete reprinting of the debates only nine years ago, a comparison of his work with Heckman's is inevitable. Both books include all the important speeches of Lincoln and Douglas in 1858 as part of the debates. Angle prefaced each with a short introduction. Heckman, instead of copying them word for word, analyzes each in turn. This reduces the wordage necessary for reprinting them by 356 pages—a tidy saving for many readers.

Both books outline the political situation during the 1850's. Angle, with many quotations from Lincoln's masterful speeches, achieves the best literary effect. Heckman, intent on giving Douglas equal recognition, presents a thoroughly objective account, though his excess of details often detracts from the readability of his text. He discusses the conflicting divisions of the Democratic party, the nation's interest in the Illinois campaign, and the bitter infighting among the Republicans. He investigates both the records and the rumors concerning attempts to enlist Douglas in the new party. His concise account of the first publication of the debates in book form is the best and most complete summary in print.

Heckman quotes the different opinions of writers concerning several moot points, and he traces interesting forerunners of Douglas' "unfriendly legislation" doctrine. His own opinions are generally guarded and unobtrusive. He is a thorough master of his subject's literature, he has tapped obscure sources hitherto neglected, and with this book he earns a prominent place among the best students in the field.

University of California, Santa Barbara

JAY MONAGHAN

SIMON CAMERON, LINCOLN'S SECRETARY OF WAR: A POLITICAL BIOGRAPHY. By *Erwin Stanley Bradley*. (Philadelphia: University of Pennsylvania Press. 1966. Pp. 451. \$7.50.)

EXCELLING in "spoilsmanship, opportunism, and political chicanery," Simon Cameron deserves much but not all of his dubious reputation. From the early 1820's until the late 1880's, his business and political ventures—the former very much dependent upon the latter—were almost always successful. Beginning as a printer and branching out into construction, Cameron received lucrative state

printing as well as canal building contracts from his friend Governor John A. Shulze of Pennsylvania. As Cameron moved into railroading, the iron industry, and banking, his political power enhanced his business interests, while these interests, in turn, influenced his political behavior. It was no accident that Cameron's one consistent political principle was the protective tariff.

Elected to the Senate as an insurgent Democrat in 1845 with the aid of Whig votes and in 1857 as a Republican with the votes of three bribed "renegade" Democrats, Cameron was notorious for winning impossible elections. Despite the hostility of his archenemy, Governor Andrew Gregg Curtin, Cameron was in the forefront of Pennsylvania Republicans and became Lincoln's Secretary of War. When charges of inefficiency and corruption, compounded by Cameron's unsavory reputation and his radical demand that the North recruit Negro soldiers, led to his removal from the cabinet and his exile to Russia, neither his eclipse nor Curtin's ascendancy was permanent. To maintain his control of the federal patronage awarded Pennsylvania, Cameron hurried home from Russia lest his machine deteriorate. With his election to the Senate over Curtin in 1867 ("a superlative example of the triumph of a hierarchial political organization over mass popularity"), Cameron became undisputed head of the Republican party in Pennsylvania. In 1877, very much the grand old man of politics, he turned over his Senate seat to his son Don and made him heir to "the most consummate political machine the Keystone State had ever witnessed."

Cameron succeeded because devoted local political leaders followed him whatever his political affiliation. Their loyalty was in part but not entirely the product of his purchasing power; Cameron did maintain a bribery-blackmail relationship with many candidates for the state legislature who needed his financial aid as much as he needed their votes. Cameron's followers also knew that with his great political acumen he was a winner who could and would reward his supporters. His lieutenants had prestigious federal offices while Curtin could offer his helpers only state offices.

This book is valuable for its delineation of Pennsylvania politics—the "game without rules"—in general and the Cameron-Curtin feud in particular. That this biography is not more enlightening on a national level reflects Cameron's failure to influence national policy. He made only one major speech (opposing the Walker Tariff), had no impact on the decision to relieve Fort Sumter, and had no significant effect on Reconstruction. Despite the subtitle "Lincoln's Secretary of War," Bradley's conclusion does not go beyond Fred Shannon's verdict that Cameron was "the weakest cog in the federal machine." This biography merely skims the surface of the vast materials on the important months when the Civil War began and Cameron headed the War Department.

Pennsylvania State University

ARI HOOGENBOOM

VAN DORN: THE LIFE AND TIMES OF A CONFEDERATE GENERAL.

By *Robert G. Hartje*. ([Nashville, Tenn.:] Vanderbilt University Press. 1967. Pp. xiii, 359. \$8.95.)

THE ebullient Earl Van Dorn was one of the Confederacy's most prominent second-line generals. Plantation-reared, the diminutive Mississippian graduated

near the bottom of the West Point class of 1842. He won distinction in Mexico and against the red men. Assuming Confederate command, he accepted the surrender of Union forces in Texas. In his two greatest battles as army commander he was defeated at Pea Ridge, Arkansas, in March 1862 and at Corinth, Mississippi, in October 1862. In lesser command, Van Dorn capably repulsed the Federals at Vicksburg in the summer of 1862, and, in a brilliant cavalry raid, turned back U. S. Grant's overland campaign against that Confederate Gibraltar by capturing Grant's base at Holly Springs in December of that year. Two months after a victory at Thompson's Station in March 1863 Van Dorn was shot to death by a personal enemy while seated at his headquarters desk. He was forty-two.

The author, Robert Hartje, is better on the commander's generalship than on Van Dorn the man, and he is more effective in discussing "The Life" than he is the "Times" of his subject. He points out that Van Dorn was more successful as a commander of mounted men than as an army commander. In the most detailed sections of the book, dealing with Van Dorn's defeats at Pea Ridge and Corinth, the author shows that, although not lacking long-range strategic planning ability, the general was often careless regarding tactical details, and that his two major defeats were caused by this shortcoming as well as by slipshod reconnaissances, the division of his forces, and the loss of proper control of his army.

Although not a profound or distinguished biography, this adequately researched study of the courageous but erratic southern general has been long awaited. There are a few factual errors, and the scholar would like a little heavier documentation in footnotes, but Hartje has been handicapped by the paucity of Van Dorn papers. At times overly dramatic, the style is often clumsy, stilted, and cliché-ridden. While the maps are helpful, a few more would have been useful, and the uncritical bibliography should have been broken down into categories.

Although containing the limitations mentioned, and being perhaps a trifle too hard on Van Dorn, this biography should remain a standard one of this controversial Confederate commander for some time.

Pennsylvania State University

WARREN W. HASSLER, JR.

ROBERT E. LEE: THE COMPLETE MAN (1861-1870). By *Margaret Sanborn*. (Philadelphia: J. B. Lippincott Company. 1967. Pp. xiv, 430. \$8.95.)

Miss Sanborn's focus in this volume, as in the one that preceded it, is on Lee the man. In depicting Lee's Confederate career and his life after the war she emphasizes the virtues attributed to him by Douglas S. Freeman, Gamaliel Bradford, and other writers. These include his intelligence, integrity, devotion to duty, unselfishness, compassion, unpretentiousness, and tolerance. Sanborn also stresses Lee's audacity, his ability to command the respect and confidence of his soldiers, his affectionate relations with his children, his quiet humor, and his kindness to animals. One quality that she develops more fully than prior biographers is Lee's fondness for women, especially those who were young and

pretty. About the only shortcoming that she finds in her subject is an occasional flare of temper.

In pointing up Lee's merits as a Confederate general the author is sometimes overly critical of his associates. She states that Jefferson Davis "looked on Lee as his most formidable rival," and she represents the Confederate President as dealing ungenerously with the general, particularly in the early part of the war. In my opinion she is also unduly hard on Joseph E. Johnston and James Longstreet. She gives no hint that ambiguity of Lee's orders may have had something to do with the tardiness of Stuart's arrival at Gettysburg.

Sanborn is more impressive when dealing with Lee's postwar career. She points up the grandeur that he displayed in accepting defeat and praises him for his precept and example in allaying the passions created by war and helping his native region play a constructive role in a reunited nation. He advised one woman who had been embittered by loss of a soldier-husband: "Madam, do not train up your children in hostility to the government of the United States. Remember, we are one country now. Dismiss from your mind all sectional feeling, and bring them up to be Americans."

In her bibliographical note the author indicates that she has read many manuscripts, including the large collection of Lee's papers in the Library of Congress. But most of her reference notes, listed by chapter in the back of the book and with no identifying numbers to help the reader match sources with text, are to printed works.

On the whole the volume is disappointing. Its tone is too laudatory, and it adds little that will contribute to better understanding of Lee.

Emory University

BELL I. WILEY

"BLACK JACK:" JOHN A. LOGAN AND SOUTHERN ILLINOIS IN THE CIVIL WAR ERA. By *James P. Jones*. [Florida State University Studies, Number 51.] (Tallahassee: Florida State University. 1967. Pp. xvii, 314. \$8.50.)

JOHN A. Logan began his political career as a prosouthern anti-Negro Democrat and ended it as one of the leaders of the Radical Republicans. This book carries the story of his career to his election to Congress as a Republican in 1866.

With the secession of the southern states Logan, who represented "Egypt" in southern Illinois with its cultural, economic, and family ties with the South, faced a dilemma. Unfortunately, Logan did not become a supporter of the war at the outset, and his position and activities in the spring of 1861 are particularly obscure. While Logan apparently cleared himself of the charge of recruiting for the Confederacy, he had to defend himself in every subsequent election, and his efforts as late as 1881 to disavow his opposition to the war and his sympathies for the South are not convincing.

It is difficult to see Logan as a leader of public opinion to the extent that he is credited by the author because he was clearly an opportunist. Before each decision to change course was made, there were long periods of silence while Logan was making up his mind which way to jump. The author is probably correct in believing that the paucity of information about Logan's more con-

troversial activities is due to his wife, Mary, who seemingly tampered with the evidence to shield her husband's reputation.

The extent of the peace movement, political developments, and attitudes in southern Illinois are given relatively little attention. More than half of the book deals with major campaigns of the war as experienced by Logan and the men under his command. In drawing on reminiscences and official sources the author has created an unusually vivid picture of the hardship, the dirty business, and yet the fascination of war.

Logan's mistakes, numerous charges against him, and his differences with McClernand, Blair, Thomas, and Sherman are revealed, but the author's sympathies are with "Black Jack." Logan gets the benefits of the heroics and the battlefield quotations, but there is little doubt of his physical courage or his popularity with his men. In spite of his unseemly eagerness to gain honor and recognition and his use of the war to further his political ambitions, the twice-wounded Logan established himself as a courageous defender of the Union and the ablest of the nonprofessional officers.

Kent State University

HARRIS L. DANTE

FEDERALISM IN THE SOUTHERN CONFEDERACY. By *Curtis Arthur Amlund*. (Washington, D. C.: Public Affairs Press. 1966. Pp. vi, 140. \$4.50.)

AMLUND feels that conceivably within every war certain forces foster centralization and give national direction to the conflict, but he does not seem willing to concede that this centripetal tendency could, and often does, accentuate, strengthen, and solidify existing attitudes, concepts, and inclinations of a centrifugal nature. His view that "southerners were compelled by wartime exigencies to increase the powers of the central government far beyond what was intended originally" would probably cause neither Jefferson Davis nor Zebulon Vance even to raise an eyebrow.

But does his view change the nature of the Confederate government or alter the disruptive influence of states' rights? It seems quite natural that the southerners should believe they had created a union of states; that was exactly what they thought the old Union was. Even if others thought it something different, the southern states did temporarily sever their connections with it. Though the seeds of federalism may have been in the Confederate Constitution under which the Congress and the executive zealously and jealously guarded their separate powers, the weed, or flower, of states' rights continued to flourish. And it is doubtful that any real student of the Confederacy would deny that certain wartime accretions of power, such as conscription by the central government, colored relations and made more difficult full cooperation among the various governmental units.

This is a dull, pedestrian, and unrewarding synopsis of the activities of the executive, legislative, and judicial branches of the Confederate government, of the relations between the state and central governments, and of the activities of the state governments. Many times retold, this story almost invariably has been written with more grace, depth, and sophistication.

Some of the discussion is misleading, and many of the "evaluations" are

ridiculous or inane. Two examples must suffice: "If the general government within a federal system is to function effectively in its sphere of action, it needs a reasonably complete administration"; and "It is not comprehensible why Americans of a century ago were preoccupied with the locus of sovereignty, because in both the Union and the Confederacy the central and state governments were working partners in an on-going system."

There is neither bibliography nor index. With much more study, analysis, and attention to expression this volume might have been turned into a good essay.

Indiana University

CHASE C. MOONEY

BALLOTS AND FENCE RAILS: RECONSTRUCTION ON THE LOWER CAPE FEAR. By *W. McKee Evans*. (Chapel Hill: University of North Carolina Press. 1967. Pp. x, 314. \$7.50.)

THIS winner of the American Association for State and Local History Manuscript Award for 1966 focuses on the eight southeastern North Carolina counties lying along the lower reaches of the Cape Fear River. Some comparisons of events in that area during the years 1865 to 1877 are made with simultaneous occurrences in the Piedmont of North Carolina and in other areas.

A rice planting and naval stores region revolving about Wilmington and for the latter part of the Civil War the most important Confederate door to the outside world, the Lower Cape Fear did not develop bitter domestic divisions and hatreds until near the end of that conflict. Rice culture had produced a stable, hereditary, and cultured gentry, but the war had ruined the cereal production and virtually destroyed the gentry. Slavery was gone, menial jobs remained, and the views of many of the Union officers tended to move toward those of the southern conservatives.

By means of the Black Code, validation of land titles, the "orphanage system" of labor supply, and other devices, the conservatives had re-established control by the end of 1865. Concurrently, Negro leadership began to emerge, and there were three Negro riots during presidential reconstruction. The conservatives underestimated the growth of political consciousness of the freedmen during the first two postwar years; nor did they seem to realize that the prewar social and economic foundations of their control no longer existed. The freedmen participated actively in Radical Reconstruction, but that period saw no assault on the concentration of economic power, which had shifted to the businessmen. The lack of homogeneity of this group—some native, some New Englanders one generation removed, and some new migrants from the Northeast—probably explains why the Republicans bypassed most of their prominent local men and nominated politically obscure individuals for office. Politics was often a matter of business and not of convictions. Despondency and cynicism overtook the Negro by 1875: he had voted, but otherwise he did almost what he had done before emancipation, and no longer did he think it "worthwhile to walk thirty-seven miles to vote for the party of freedom."

Evans has used his sources well; his short biographical sketches are good; the socioeconomic analyses are meaningful; and his writing is clear and clean,

though at times a trifle flippant. In this good book he shows beyond doubt that the right to vote cannot alone diffuse political power and that the patterns of Reconstruction at the local level were not always the same as those at the state level.

Indiana University

CHASE C. MOONEY

SALT WATER & PRINTER'S INK: NORFOLK AND ITS NEWSPAPERS, 1865-1965. By *Lenoir Chambers* and *Joseph E. Shank*, with a final chapter by *Harold Sugg*. (Chapel Hill: University of North Carolina Press. 1967. Pp. viii, 418. \$7.50.)

THIS centennial history of Norfolk newspapers dates from the founding of the Norfolk *Virginian* on November 21, 1865, "the first native, independent newspaper to be established . . . [there] after the Civil War," ancestor of the present *Virginian-Pilot* and its evening partner, the *Ledger-Star*. Since 1933 they have been published by Norfolk Newspapers, Inc., whose present publisher, Frank Batten, engaged the authors to write the history. Joint authorship evolved from teamwork in research and writing. Joseph E. Shank examined the files of the major papers, and of many minor ones, page by page, "to record information relating to the newspapers themselves and to events that were significant in their broader history," reducing the vast wordage of the whole to "approximately a million words of notes in narrative style." Lenoir Chambers, Pulitzer Prize-winning editor of the *Virginian-Pilot* and biographer of Stonewall Jackson, wrote the history, based upon Shank's digest, supplemented by numerous other primary and secondary sources, but without annotations.

"Newspapers are an organic part of the life around them," writes Chambers in his preface. "A story of newspapers cannot help being a story of cities and regions, and in some degree of the nation." He demonstrates this point so effectively in his narrative that the reader finds himself pleasantly engaged in a social history of Norfolk and its salt-water environs. This is not to imply that Chambers gives other than first consideration to the "printer's ink" in his title. In numerous ways the history of Norfolk's newspaper press epitomizes the fat years and the lean ones of this seaport, which began life anew in 1865, struggled to achieve economic stature through its commercial resources, domestic and foreign, experienced fluctuations of population geared to the United States Navy and nearby navy yard, and became the major part of Virginia's largest urban center by mid-century.

These economic and social ingredients of Norfolk's history have leavened its political life and nurtured the two-party system that has been woefully lacking in the commonwealth since the Civil War. As newspapermen inevitably become involved in political issues, so the Norfolk press tells both sides of the story of political Reconstruction, of readjusters versus conservative Democrats, the rise of the Republican party, and the issues of local politics involved in the growing pains of an urban community. Norfolk has had its full share of colorful journalists who enliven the narrative: Irish-born Michael Glennan, scholarly and poetic James Barron Hope, mercurial and courageous William C. Elam,

urbane and liberal-minded Louis I. Jaffé, and many others. Thumbnail sketches of numerous minor figures will hold the reader's attention, as, for example, in the chapter, "Who Scooped the World?" on the Wright brothers' flight at Kitty Hawk. Throughout the book Chambers maintains commendable perspective on the main theme so that the reader fully comprehends the transformation wrought in American newspaper journalism during the past century from the heyday of the editor to the dominance of the manager. The numerous illustrations are well selected, and the index is excellent.

Institute of Early American History and Culture

LESTER J. CAPPON

AT EASE IN ZION: SOCIAL HISTORY OF SOUTHERN BAPTISTS, 1865-1900. By *Rufus B. Spain*. (Nashville, Tenn.: Vanderbilt University Press. 1967. Pp. xiii, 247. \$6.95.)

THIS is an interesting addition to the expanding literature on southern Protestantism in the post-Civil War period. Concentrating on the social attitudes of one of the major denominational groups in the South, Spain discusses in separate chapters the response of the Baptists to the Negro "problem," temperance, economic questions, politics, and questions of personal morality. His findings reveal the overwhelmingly conservative attitudes of the southern Baptist churches throughout the late nineteenth century. The author presents his case convincingly and, at the close, succinctly: "Their [the Baptists] significance in Southern life consisted not in their power to mold their environment. . . . Rather their importance as a social force was in supporting and perpetuating standards prevailing in society at large."

The principal weaknesses of this study flow directly from a rather narrow base of research. Professor Spain has chosen to devote almost all his attention to the formal records of the southern Baptists and to their numerous denominational papers. A broad historical context for his findings is missing. The interplay of differing attitudes North and South on social issues both within the Baptist denomination and between Baptists and other religious groups is hinted at, but never adequately revealed. Most important, the author fails to view his subject within the framework of evangelical Protestantism, of which southern Baptists were excellent representatives. In the last chapter, for example, Spain usefully though all too briefly relates southern Baptist attitudes on social issues to the highly individualistic ethical system of the evangelicals. More could be done to show the implicitly conservative social outlook engendered by individualist ethics in an age of growing interdependence, a fact affecting evangelicals in the North as well as the South. Unfortunately, too, the author's bibliography offers few adequate clues to specialized materials for those who might seek to learn more about this background for themselves. Spain's work is helpful, but it is to be hoped that similar studies in the future will cast the net of research a bit more widely in order to provide readers with a fully drawn historical image.

De Pauw University

JAMES FINDLAY

JESSE JAMES WAS HIS NAME: OR, FACT AND FICTION CONCERNING THE CAREERS OF THE NOTORIOUS JAMES BROTHERS OF MISSOURI. By *William A. Settle, Jr.* (Columbia: University of Missouri Press. 1966. Pp. 263. \$6.00.)

JESSE and Frank James are surrounded by myth. Their lives have produced a vast literature of biographies, scenarios, ballads, and novels. We know that they were Missourians and that they were among Quantrill's and Bill Anderson's guerrillas, some of whom turned to crime after the Civil War. Once we thought that these brothers were desperate criminals, guilty of many midwestern robberies and murders. The account of the "Notorious James Brothers" by William A. Settle, Jr., creates doubts that our previous knowledge was based upon fact. But Settle is probably right when he questions whether fact can change the popular view of the James brothers. Commenting upon another's work, and indirectly upon his own, Settle asserts that "the James legend . . . is too well rooted in American folklore to suffer serious damage."

With painstaking care Settle examines all evidence relating to the James brothers; he questions what others have offered as fact and presents available historical evidence. From 1869 to 1882, the author neither proves nor disproves that the James brothers participated in the bank and train robberies usually connected with them and their associates. Evidence found in newspapers, court records, purported eyewitness accounts, public documents, manuscript collections, memoirs, and reminiscences is so conflicting that no future writer can claim to be a historian and maintain that the James brothers' activities can be established with certainty. Settle strips off the veneer of alleged fact and shows that little is known conclusively about the James brothers' careers.

As former Confederates, the Jameses enjoyed their ex-comrades' deepest sympathy. Not only did they pose criminal problems, but Settle reveals that they created political problems as well. Until 1885, when the last criminal charge against Frank James was dismissed in court, Missouri Democratic and Republican politicians used the brothers' careers to win votes. The volume is so tightly written that some will object that other volumes about the Jameses are more enjoyable reading. But when comparisons are made, the authors of other accounts will be contributors to legend and myth, and Settle will emerge as a historian.

University of Oklahoma

DONALD J. BERTHRONG

THE BUFFALO SOLDIERS: A NARRATIVE OF THE NEGRO CAVALRY IN THE WEST. By *William H. Leckie.* (Norman: University of Oklahoma Press. 1967. Pp. xiv, 290. \$5.95.)

THE distinguished quarter century of service (1866-1890) on the Indian fighting frontier by the Ninth and Tenth Cavalry—whom the Plains Indians called "buffalo soldiers" because of the texture of their hair—has long gone unrecorded save for a few pages in *The Army of the United States* (1896), edited by Theophilus F. Rodenbough and William L. Haskin, and a deplorably error-riddled history of the Tenth. But this is not surprising since until recently the American

Negro has usually been treated merely as a "problem." Histories of the Negro are revealingly shelved in the sociology section.

The Buffalo Soldiers records, principally from materials in the National Archives, the struggles of Negro soldiers and their white officers in the Far West during a period when for months and even years at a time they were the sole garrisons of many frontier posts. Their enemies were not only hostile Indians and Mexican and white desperadoes but also murderously bigoted white citizens, including Texas Rangers and jurymen who never found a white man guilty of any offense against a Negro. Prejudice also manifested itself in stationing Negro regiments without intermission in regions of the most grueling climate and supplying them with poor food and equipment and little praise. Yet, on the whole, the morale of the "buffalo soldiers" was high and their record of achievement excellent, partly because, even as today, army life still offered more opportunities to the ambitious and able but uneducated Negro than did a Mississippi plantation or a northern slum.

Criticisms are minor and peripheral. Since the Twenty-fourth and Twenty-fifth Infantry, also Negro, served closely with the Ninth and Tenth Cavalry they logically should have been included in the story of the "buffalo soldiers." It is odd, too, that the author seems unaware that the frequently mentioned Seminole scouts were actually Negroes. Illustrations are a weak point. The portraits are all of white officers, and the other photographs are not very effective. Some of Frederic Remington's many drawings of "buffalo soldiers" might well have been used, in addition to the one full-page drawing, which is not credited to Remington.

There may still be room for more detailed treatment of particular campaigns and special aspects, but no general history of the Negro cavalrymen who fought the Indians will soon be needed. This volume will be welcomed by all interested in either American military history, the West, or the Negro.

University of Oregon

KENNETH WIGGINS PORTER

PENNSYLVANIA POLITICS, 1872-1877: A STUDY IN POLITICAL LEADERSHIP. By *Frank B. Evans*. (Harrisburg: Commonwealth of Pennsylvania, Pennsylvania Historical and Museum Commission. 1966. Pp. vii, 360. \$5.00 postpaid.)

It seems fitting for this book, with fine illustrations and a useful index, to appear currently as Pennsylvania considers revising the Constitution written and approved in 1873. In spare, scholarly description, citing Pennsylvania newspapers and pertinent manuscripts, Evans explains the why as well as the what of all levels of Pennsylvania politics in a period when there was almost constant political campaigning. Since this era was Republican dominated, the emphasis is on that party, but the Democrats and the third parties are not neglected. Worth reading is the author's analysis of newspapers in the state and of the leaders of the Philadelphia and the Allegheny County rings.

Pennsylvania's importance in national politics is not overlooked, and the disputed election of 1876 makes that importance dramatically clear. Attention is focused, naturally, on the role of the two most prominent Pennsylvanians in

the crisis: Secretary of War J. Donald Cameron, son of boss Simon Cameron; and Sam Randall, Democratic Speaker of the House of Representatives. Certainly their part in the electoral impasse has previously been underplayed.

Evans believes that Republican ascendancy should be ascribed to Pennsylvania's adherence to two principles: the Union and the protective tariff. Devotion to these issues and firm loyalty to his friends gave Simon Cameron his hold on the state, a hold that on his retirement enabled him to pass on the mantle of leadership to his son. Republican victory in the state also reflected the lack of an effective opposition program and the bitter Randall-Wallace feud for control of the Democracy.

No matter how entertaining present-day politics may seem, dead politicians in overwhelming numbers do not make for the liveliest reading. Nor is all the information presented entirely new. For the most part, however, Evans succeeds in his task; one may disagree with some explanations, but the results of his careful research should prove valuable both for Pennsylvanians and those assessing the national scene. He has demonstrated that post-Civil War Pennsylvania Republicans found in the Camerons what they wanted, not statesmen but master politicians. It would require many decades for the pattern established then to be broken.

American University

DOROTHY D. GONDOS

THE PAPERS OF WOODROW WILSON. Volume II, 1881-1884. *Arthur S. Link et al.*, Editors. [Sponsored by the Woodrow Wilson Foundation and Princeton University.] (Princeton, N. J.: Princeton University Press. 1967. Pp. xvi, 680. \$15.00.)

THIS second volume of the definitive *Papers of Woodrow Wilson* upholds well the outstanding promise of the series. Meticulously and intelligently edited by a team led by the nation's authority on Wilson, it displays at every turn of the page the accuracy and the comprehensive character that scholars must demand of such an effort. But it goes much farther. Both the documents themselves and the explanatory footnotes add significantly to our understanding of Wilson, and they suggest some fundamental revisions. As the editors point out, one can no longer jump to the conclusion that the young Wilson was a failure in the practice of law. Again, the letters betray quite enough of the young man's self-consciousness to make it clear that he understood his own limitations more clearly than many of us have realized. Consider this passage from a love letter to his "precious Ellie": "Somehow I always found it hard to believe that anyone could fall in love with me—not because I had a poor opinion of myself, for I have always entertained a quite high enough estimation of my own gifts. . . . But . . . I knew that I was not good looking enough to attract admiring attention, and I was conscious of wearing, towards all but a few intimates, a cold exterior which was no more likely than my limited conversational powers to win favour from the fair sex." If there are some gentle surprises, other images are confirmed. One sees early the elements of the troubled spirit which has so preoccupied recent Wilson scholarship. And one sees earlier than one might have expected the ideas that later became well known in *Congressional Government*. Both the toughly

realistic view of political processes and the hopelessly romantic notions about human relationships are betrayed early. But most important, one sees Wilson as he really was. His views on race, for example, seem shocking. But the fact that they do appear shocking will be a renewed warning of the extent to which we have wrenched Wilson from his own world and tried to appraise him with the criteria of our day.

If the *Papers* continue to appear at this level of quality, we shall have Arthur Link and his colleagues to thank for one of the classics of this generation.

University of Vermont

ALFRED B. ROLLINS, JR.

THE PENNSYLVANIA GERMANS, 1891-1965 (FREQUENTLY KNOWN AS THE "PENNSYLVANIA DUTCH"): SEVENTY-FIFTH ANNIVERSARY VOLUME OF THE PENNSYLVANIA GERMAN SOCIETY. By *Homer Tope Rosenberger*. [Volume LXIII, the Society.] ([Allentown:] the Society. 1966. Pp. 619. \$12.50.)

THE author of this volume shows what the Pennsylvania Germans have accomplished in the years 1891-1965 and how they responded to the national change from rural to primarily urban life. The book constitutes almost an encyclopedic record of the attainments of these people during the past seventy-five years. It begins with a brief history of the Pennsylvania Germans up to 1891, proceeds to a chronological narrative of their participation in modern American life, and concludes with topical chapters on the Pennsylvania German Society, on the literary and scholarly activity of the Pennsylvania Germans, on their adjustment to modern agriculture, on their changing dialect, dictionaries, and customs, and on the major collections of source materials currently available to scholars. Dr. Rosenberger has, in addition, included two biographical chapters, one covering Nobel and Pulitzer Prize winners, and the other giving capsule biographies of several hundred representative Pennsylvania Germans who attained eminence.

This handsomely produced volume contains over a hundred halftone and some color prints and a very complete index but no footnotes or bibliography. Chapter XII, "Analysis of the Literature of the Pennsylvania Germans in These Seventy-Five Years," serves, however, as a working bibliography with the materials listed by year of publication. The author incorporates many of his sources in the text and has acquired considerable contemporary material directly from nearly a hundred consultants.

Rosenberger writes with pride and appreciation of the Pennsylvania Germans. It is understandable that he deals primarily with their attainments and successes and avoids their frustrations and failures. The book, thus, is not a critical study but rather an engagingly written narrative combined with a carefully crafted documentary record. It provides a valuable and lasting account of the work of the Pennsylvania Germans since 1891 in the arts, scholarship, science, invention, business, agriculture, government, education, religion, philanthropy, and nearly every aspect of national life with the exception of sports, a term not included in the index.

The author has succeeded in his purpose of providing a bench mark from which future historians can measure changes in Pennsylvania German society.

He has brought up to date a story too often known only in the context of early American history.

Pennsylvania State University

PHILIP S. KLEIN

THE BILLY MITCHELL AFFAIR. By *Burke Davis*. (New York: Random House. 1967. Pp. 373. \$7.95.)

"BILLY" Mitchell won his brigadier general's star and fame as General Pershing's Chief of Air Service, First Army, and Chief of Air Service, Group of Armies, in France in 1917-1918. His court-martial in 1925 for insubordination was one of the sensations of the Coolidge era. Labeled "fanatic" by those contemporaries with conventional views on air power, he was upgraded to major prophet in 1940-1941 when the air war in Europe and the attack on Pearl Harbor appeared to vindicate his most extreme predictions. Burke Davis, well-known novelist and biographer, delves into newly opened personal and official files to trace effectively the road that led to Mitchell's trial and to describe vividly the views and events that made Mitchell a hero and a legend in his time.

As the title of the book indicates, Davis emphasizes Mitchell's fight for increased air power against the proponents of ground forces and battleships in the United States Army and Navy and the court-martial that resulted from his ferocious attack on civilian and military leaders who stood in his way. Drawing on interviews with Mitchell's admirers and on the airman's writings—some of them long locked in classified files—he gives a clear picture of a zealot who asked no quarter and strode the path of vigorous evangelism for his cause. For him the longer road of diplomatic persuasion was an impossibility.

Davis is an obvious admirer of his subject. His handling of the controversy in the early 1920's of the bomber versus battleship gives the best of the argument to Mitchell, suggesting that the old navy would spare no efforts to prevent the trials off the Virginia capes from succeeding. His respect for the evidence, nevertheless, somewhat deflates the Mitchell legend. Davis admits that the airman, frustrated by the opposition of his superiors, deliberately forced a showdown by his intemperate claims and charges. He doubts, however, that he was courting martyrdom in the process. Supremely self-confident, Mitchell believed that he would be upheld once he could get a public hearing.

Some of Mitchell's strongest backers—General "Hap" Arnold was one—agreed that the airman's sentence was to be expected in view of his belligerent attitude. The decision may well have reflected the narrow views on air power held by the majority of the generals comprising the trial board, but Mitchell was found guilty on the basis of evidence that he did not deny.

Davis clearly delineates the old army's and the old navy's antipathies to new weapons and newer strategic doctrines. He also shows why a believer in air power felt that he must challenge the dead hand of tradition. He does not succeed in establishing the thesis that Mitchell advanced the cause of air power by his strong-arm tactics. Rather, Davis makes the reader understand why the British air leader, Hugh Trenchard, said of his fellow airman in World War I:

"Mitchell is a man after my own heart. If he can only break the habit of trying to convert opponents by killing them, he'll go far."

Arlington, Virginia

FORREST C. POGUE

LANDON OF KANSAS. By *Donald R. McCoy*. (Lincoln: University of Nebraska Press. 1966. Pp. x, 607. \$8.50.)

THIS competent, well-organized, and objective book is the first scholarly biography of Alfred M. Landon. Like other careful students of recent American politics, McCoy rejects the "Kansas Coolidge" image fastened to Landon following his woefully unsuccessful presidential campaign in 1936. Landon emerges as a Bull Moose in 1912, a supporter of Robert M. La Follette in 1924, and a consistent advocate of free speech and civil liberties. An "exceptionally good governor" of Kansas in the early 1930's, he played an "important part" in moving the GOP toward "modern Republicanism." Landon also appears as a competent if uninspiring campaigner, an effective worker for party unity, and an important party leader long after 1936. McCoy stresses Landon's significant role in Kansas politics until 1948 and describes a forthright elder statesman who opposed right-to-work laws, demanded the replacement of John Foster Dulles in 1958, and asked consideration of Red China's admission to the United Nations. Throughout his career Landon championed fiscal orthodoxy and opposed monopoly. McCoy regrets Landon's occasional "crankiness" and "immoderation," but he respects his independence and sense of responsibility.

The book has a few shortcomings. McCoy not only portrays Landon, but at times tries to write a history of recent Kansas politics and a chronicle of the GOP from 1935 to 1948. Much of the material on Kansas is new and important; some of the sections on national affairs are not. Both preoccupations tend to submerge his subject in party maneuver. McCoy also includes much unnecessary detail, quoting speeches at great length and deluging the reader with day-by-day accounts of Landon's varied activities. A little less chronicle and more analysis would have improved the book. Except for a brief foreword, a few closing paragraphs, and a fine chapter analyzing the 1936 campaign, the book usually lets the wordy record speak for itself.

Several strong points more than balance these weaknesses. The author diligently mined some voluminous manuscript collections, especially those of William Allen White, Frank Knox, and Landon himself. (The papers of Frank Gannett and Arthur Vandenberg would have added more in places.) Interviews with Landon and his associates helped clarify important questions. The chapters on Landon's governorship and on his emergence to candidacy in 1936 are especially convincing.

Indiana University

JAMES T. PATTERSON

A STRIPE OF TAMMANY'S TIGER. By *Louis Eisenstein* and *Elliot Rosenberg*. (New York: Robert Speller and Sons. 1966. Pp. xii, 300. \$5.95.)

Louis Eisenstein was a Tammany election district captain on Manhattan's Lower East Side from the 1920's to the early 1960's. In this personal history, written

with the help of a former newspaperman, he lovingly re-creates the vanishing "game" of neighborhood politics. The anecdotes he tells are entertaining and sound authentic, but are undocumented. Like other reminiscences based on memory, hearsay, and strong opinions of men and affairs, this one must be read with caution.

It is very useful, though, for measuring the size of the average precinct worker in Tammany Hall. Men like George Washington Plunkitt rose to be assembly district leaders and made much money through their connections, but the Louis Eisensteins were content to remain election district captains and to live off patronage crumbs all their lives. Such men—there were around 3,500 of them when Eisenstein joined a district club 50 years ago—regularly delivered the vote for Tammany, Eisenstein says with pride, no matter who the candidates were or what they stood for.

He further makes the point that the stakes of politics are, and ought to be, jobs and favors. The election district captain looks after his voters, and the assembly district leader takes care of his election district captains. What held the system together, Eisenstein emphasizes, were men who placed the highest good on keeping their word.

There is much to be said for that virtue, as there is for personal loyalty and Tammany's concern for poor immigrants. Yet the parochialism of neighborhood politics set serious limits to the hall's sense of honor and humanity. A single broken promise to a job-seeker disturbs Eisenstein more than wholesale corruption, incompetence, and actual stealing. He never in his life, moreover, thought of improving the East Side; in fact, he sneers at efforts to use politics as an instrument for social change. Even now, in the mellowness of retirement, he derides candidates for office with first names like Bentley or Newbold. No one of any honor bore such "uptown" names when John F. Ahearn and his sons Eddy and Billy ran the Lower East Side's old Fourth Assembly District with the help of the Louis Eisensteins.

University of Chicago

ARTHUR MANN

FATHER O'HARA OF NOTRE DAME: THE CARDINAL-ARCHBISHOP OF PHILADELPHIA. By *Thomas T. McAvoy, C.S.C.* (Notre Dame, Ind.: University of Notre Dame Press. 1967. Pp. xi, 514. \$7.50.)

To Father McAvoy, professor of history in and archivist of the University of Notre Dame and a member of the Congregation of Holy Cross, was assigned the delicate task of writing the biography of John F. O'Hara (1888-1960), an older colleague who became president of the university (1934-1939), auxiliary bishop and vicar-general to Archbishop Francis Spellman in the Military Ordinariate, that is, as ecclesiastical superior of Catholic chaplains in the armed forces (1940-1945), bishop of Buffalo (1945-1951), archbishop of Philadelphia (1952 until his death), and a cardinal (1958). Although a work of edification was apparently expected, a scholar such as the author could not be content to produce a book devoid of historical value. Hence, he obtained access to most of the pertinent sources and made judicious use of them, especially

where persons still living are concerned; he also gathered the recollections of surviving contemporaries of his subject. Adroitly eschewing any explicit statement of his own opinion, McAvoy often simply quotes others to express criticism of O'Hara and lets the reader draw the conclusions for himself. In his objective presentation of the facts the writer adheres to such a strictly chronological order that he keeps returning to certain themes while interspersing other material; a topical arrangement within each chapter would have resulted in a more unified and concise treatment. There is an index of proper names only; it is, unfortunately, incomplete.

Since the biographer has not attempted to determine the significance of O'Hara's life for the United States or for the American Catholic Church, I may be permitted some general comments. Zealously dedicated in all his activities to the cause of his Church as he understood it, this humble and pious ecclesiastic is chiefly to be remembered as a diligent promoter of Catholic education. Although he was exposed to advanced studies in history under Peter Guilday, he never earned a doctorate, for he preferred more practical subjects such as commerce and economics and enjoyed his early position of spiritual adviser to the students. Intelligent but anti-intellectual, as president of Notre Dame he defended the predominance of athletics and combed the library stacks for books that he deemed dangerous to faith and morals, and yet he began to introduce graduate programs. Later he strained the personal and financial resources of the diocese of Buffalo and the archdiocese of Philadelphia to multiply and enlarge Catholic schools, especially high schools, and he was intolerant of doubts about his idealistic goals. He was firmly opposed to federal aid to education and desired no tax support for Catholic schools. Having spent three years of his youth in Uruguay and Brazil, where his father was a consul, and having retained his interest in that area throughout his life, he was widely regarded as an expert on Latin American affairs. In purely religious matters he attached great importance to statistics, such as numbers of confessions and Communions. He frowned on the liturgical movement and prohibited any participation in interdenominational undertakings. He also abhorred the discussion of racial problems; as president of Notre Dame he refused to admit Negro students, and he attributed Negro agitation in general to incitement by the Reds. As ardent in his anti-Communism as in his patriotism, he befriended many champions of the far Right. He was a persistent critic of the Social Action and Education Departments of the National Catholic Welfare Conference, but he seems to have exerted little influence on most of his fellow bishops.

When other churchmen noted for positive and farsighted leadership during the same period are still neglected by historians, one may ask whether O'Hara deserved a full-length biography. The book is useful, nevertheless, as documentation of a prominent case of episcopal conservatism before the Second Vatican Council, the spirit of which in many respects he would have found uncongenial.

Catholic University of America

ROBERT TRISCO

CONGRESSIONAL CONSERVATISM AND THE NEW DEAL: THE GROWTH OF THE CONSERVATIVE COALITION IN CONGRESS, 1933-1939. By *James T. Patterson*. ([Lexington:] University of Kentucky Press for the Organization of American Historians. 1967. Pp. ix, 369. \$8.50.)

THIS volume, which won the Frederick Jackson Turner Award of the Organization of American Historians in 1966, provides a competent account of the actions of Capitol Hill conservatives during Roosevelt's first six years and an analysis of the bipartisan conservative coalition that came to dominate Congress. The work is based upon wide research in manuscript collections, congressional materials, and the contemporary press. The roughly chronological treatment of the main issues that divided President and Congress gives us a useful account of the main events of national politics. Conservatives in Roosevelt's first Congress had little recourse but to grumble and bide their time, but in the next Congress they were able to make their power felt a little and to make much more noise even though the 1935 session of this Seventy-fourth Congress enacted the most ambitious reform program of the century. Despite a new low for Republican representation after the 1936 elections, conservatives on the Hill snapped back and substantially dominated the next Congress, thanks to Roosevelt's disastrous court proposal. The conservatives were able after the 1938 elections to forge a coalition that dominated Congress for the next several years.

Congressional Conservatism is well researched and clearly written, and its general conclusions are beyond serious question, but a close reading reveals confusion at points. The root of the difficulty is Patterson's strong tendency to use resistance to Roosevelt as his working definition of conservatism. Certainly conservatives opposed the President, but not all his opponents on every vote were conservative. In an appendix the author lists the most conservative congressional Democrats and explicitly makes opposition to the administration on roll call votes his criterion of conservatism. By this method, a vote for the Black thirty-hour bill counts as a conservative mark. The same is true of a vote against the World Court, although Patterson notes in the text that divisions over foreign policy cut across the usual Left-Right lines. This conservative-liberal accounting system gets fuzzier on the Wagner labor bill of 1935. FDR never declared himself for the labor bill until after all roll call votes, and he was apparently at least passively opposed to it, but a vote for it is counted as liberal, which makes sense actually, but does not fit the explicit definition. Similarly, Patterson counts a vote for the Wagner-Steagall Housing Act of 1937 as liberal, although he says in one place that FDR never conceived of it as an administration measure and in another says that the bill's "final version largely reflected the views of conservatives." Despite flawed logic, however, Patterson's book is a significant contribution to an important subject.

University of Maryland

DAVID SHANNON

THE CIVILIAN CONSERVATION CORPS, 1933-1942: A NEW DEAL CASE STUDY. By *John A. Salmond*. [Duke Historical Publications.] (Durham, N. C.: Duke University Press. 1967. Pp. vi, 240. \$6.50.)

CONFRONTED with 55 per cent of the nation's youth unemployed or working part-time, one sixth of the country's fertile soil turned to dust, and the forests virtually denuded, Franklin D. Roosevelt quickly responded with the Civilian Conservation Corps. His long-time dedication to conservation of human and natural resources made this move natural and inevitable.

In this incisive, interpretive case study, John A. Salmond, senior lecturer in history at Victoria University in New Zealand, has contributed a meaningful, sympathetic, yet objective appraisal of the CCC. He has absorbed doctoral dissertations, governmental reports, previous works on the CCC, and the vast New Deal literature. The focus of his study is the development of the corps' central organization, as seen and influenced by the chief executive, legislators, administrators, and the press.

Compromise legislation, which freed the President from many administrative restrictions, received overwhelming bipartisan support. Within three months, 275,000 young men were working in the forests and plains, a historic feat facilitated by surrender of control of the project to the War Department by a cumbersome interdepartmental arrangement. Unfortunately, CCC Director Robert Fechner, hard-working conservative trade-unionist and traditional southerner, lacked the innovation and vision to shape the organization beyond that of a relief agency. And neither he, the President, nor the American people were prepared to honor the clause that forbade discrimination in the selection of Negro enrollees. Roosevelt's interference, at times, evoked grave consequences, as when he sought to balance the budget in 1936 to gain conservative votes in November, at the expense of CCC enrollment. Fortunately, Congress revolted.

Fechner sided with the military who feared "radical and leftist infiltration" and who excluded the *New Republic* and *Nation* from camp libraries. Army opposition to "cultural courses," and to writers of sociology because they tended to be "somewhat radical," seriously restricted the potential success of an educational program. And a quasi-fascist, anti-Negro, anti-Semitic general commanded most of the southern CCC camps.

Congress abolished the CCC in 1942 because, viewed as a relief agency, its role had become outmoded in an era of full employment. The tragedy was that neither Roosevelt nor Fechner fully tapped the long-term potentialities to educate as well as conserve. But the vital contribution of the CCC remains part of our heritage, "the first attempt by the federal government to provide some specific solution for the problems of youth in an increasingly urban society." The author concludes, though without proof, that the CCC was a building block for the National Youth Administration, postwar federal aid to education, the Peace Corps, the Job Corps, and the Teacher Corps. And that deserves another equally fine work.

City College of New York

BERNARD BELLUSH

WILLIAM MORRIS LEISERSON: A BIOGRAPHY. By *J. Michael Eisner*. (Madison: University of Wisconsin Press. 1967. Pp. 144. \$6.75.)

WILLIAM MORRIS Leiserson's life as described by Professor Eisner was an atypical Jewish immigrant's success story. Born in 1883 in Russia's Baltic province of Estonia, Leiserson came to America in 1890 to live with his family in New York's Lower East Side ghetto. Leiserson's life, however, inexplicably diverged from that of his peers in public school, street society, and settlement house. Not content with a public-school education he went to college, in his case the University of Wisconsin rather than the Jewish immigrant's usual choice, City College. Trained by John Commons at Wisconsin and by other leading academic economists at Columbia where he earned a Ph.D., Leiserson made his career in the larger American, gentile community. Before long he married outside the Jewish faith, accepted a professorship at Antioch College, became a nationally renowned labor arbitrator, served state and local governments, and crowned his career in public service by reforming the New Deal's National Labor Relations Board. Eisner, utilizing fully Leiserson's ample personal papers available at the Wisconsin State Historical Society, touches upon all aspects of his subject's life with special emphasis on his role as labor arbitrator. Of particular interest to historians for what they may infer from it about the internal politics of the New Deal is Eisner's treatment of Leiserson's work on the National Labor Relations Board (Chapter VII).

A well-conceived biography of Leiserson would provide fresh perspectives on the immigrants' experience in America, the rationale of pioneer labor arbitrators, and the administrative difficulties of the New Deal. But this book offers no such insights. Too brief for a full-scale biography and too diffuse for an essay, the book substitutes platitudes and truisms for hard historical analysis. From an introduction in which Eisner refers to Richard Ely and other reform economists as an academic expression of Populism, to later chapters in which he makes Communists and intellectuals interchangeable, to a conclusion in which he explains Leiserson's successes simply in terms of the arbitrator's understanding of human nature (whatever that cliché means), similar suggestions, definitions, and notions appear regularly. Never, however, does Eisner probe beneath the surface of Leiserson's character and behavior; never does the reader discover what actually made Leiserson so different from other first-generation Jewish immigrants. Instead, Eisner in strained, tired prose renders the portrait of a dull, ideologically provincial anti-intellectual (Eisner's term). Leiserson's own life and writings, however, belie the man sketched by Eisner. The real man was lively, witty, and intellectually able, one who deserves a bigger book and a better biography.

University of Massachusetts

MELVYN DUBOFSKY

JEWS IN THE MIND OF AMERICA. By *Charles Herbert Stember et al.* Project Director, *Marshall Sklare*. Editor, *George Salomon*. Preface by *John Slawson*. [Published in collaboration with Institute of Human Relations Press.] (New York: Basic Books. 1966. Pp. xiv, 413. \$12.50.)

THE first section by Charles Herbert Stember, professor of sociology at Rutgers

University, consists of an analysis of public opinion polls taken in the United States from 1937 to 1962 concerning attitudes toward Jews. The surveys represent, in Stember's words, "a larger and more varied body of information on the subject than has been assembled in any other study. . . ." The second portion of the book consists of eleven papers commenting on Stember's findings from historical, sociological, psychological, and demographic points of view. A superb introduction has been contributed by Theodore Solotaroff, an editor of *Book Week* and Marshall Sklare, professor of sociology at Yeshiva University.

Stember's major finding is that there has been a dramatic decline in prejudice against Jews as revealed in the surveys. Historians John Higham of the University of Michigan and Morton Keller of Brandeis University adduce supporting historical evidence which purports to show that anti-Semitism—if not totally dead—is a disappearing problem in America and unthinkable as a powerful social movement.

Adopting an even longer historical view, Ben Halpern, associate professor of Near Eastern and Judaic studies at Brandeis, underlines the limitations of public opinion polls for measuring so deep and complex a phenomenon as anti-Semitism. Halpern agrees that Americans have become more tolerant, but, taking a long look at the past (including the situation of the Jews of ancient Rome and Alexandria and of post-Enlightenment Europe), sees recurring tensions ahead. One had only to follow the activities surrounding a school board election in the town of Wayne, New Jersey, in February 1967 to see substantial truth in Halpern's perspective. It was considered tasteless to say nasty things about Jews in Wayne, but in the privacy of the polling booths, open political anti-Semitism appears to have been endorsed resoundingly.

Still, the general conclusion that anti-Semitic attitudes have declined and feelings repressed over the past three decades seems sound. Many factors have been involved: the rise of Jewish strength in politics; the unspeakable horrors of Hitlerism; the birth of the state of Israel; and the closing of certain psycho-cultural gaps between Jews and non-Jews that appear to have made them more acceptable to each other. The Puritan reaction to the Jewish enjoyment of pleasure has been reduced sharply by this decline of asceticism almost everywhere in America. The gap between the Jewish emphasis on intellectuality as compared to the American stress on techniques has narrowed. Even Jewish cosmopolitanism in an age of nationalism seems less threatening as educated elites become more international in their approach.

But Halpern seems justified in criticizing Stember for an overreliance on public opinion polls. Unquestionably, attitudes and behavior have changed and perhaps even feelings, too. But anti-Semitism which persisted for hundreds of years when there was not a single Jew living in either Spain or Portugal, France or England, and many sections of Germany, ought not to be counted out yet, even in liberal America.

Brandeis University

LAWRENCE H. FUCHS

EISENHOWER AND BERLIN, 1945: THE DECISION TO HALT AT THE ELBE. By *Stephen E. Ambrose*. [The Norton Essays in American History.] (New York: W. W. Norton and Company. 1967. Pp. 119. \$4.00.)

GENERAL Eisenhower has long been castigated for presumably leaving Berlin to the Russian armies by halting Allied forces at the Elbe River in April 1945.

Making no claim to uncovering new material, Professor Ambrose, associate editor of the Eisenhower Papers, offers an able and convincing brief defending Eisenhower's decisions. He has made it more appealing with bold, muscular views on the complicated issues and the numerous actors involved.

The unexpected capture of the Remagen Bridge by American troops on March 7, 1945, markedly influenced Eisenhower to stop at the Elbe, Ambrose believes. Grasping the opportunity to cross the Rhine River, Eisenhower decided on a new plan: attack the Germans on a broad front with large forces, especially those of General Bradley's Twelfth US Army Group. His earlier plan, strongly advocated by the British for more than military reasons alone, called for a drive toward Berlin on a narrow front using Field Marshal Montgomery's limited forces.

If ordered to do so for political reasons, Eisenhower would have tried to capture Berlin, Ambrose argues. Since he was not, he intended to run a purely military campaign and avoid a possible clash with the Russians. Nor was Eisenhower about to change FDR's political decision to continue the working partnership with the Russians after the Germans were defeated, an arrangement that might have succeeded, the author asserts, had the opportunity been afforded.

That American troops could have taken Berlin first, Ambrose is convinced, was unlikely. When Eisenhower halted General Simpson's Ninth US Army, it was holding a tenuous bridgehead still some fifty difficult miles away. The Russians, in contrast, had 1,250,000 men poised to cover the final thirty-three miles of flat, dry land. The Russians took Berlin at a heavy cost, only to give up over half of it to the Western Powers two months later because of prior agreements on joint occupation. The same agreements would, naturally, have applied had the Allies taken the German capital.

Ambrose might have given more weight to Churchill's arguments that had the Allies captured Berlin, and later Prague, they might have pressured the Russians to live up to their wartime agreements, especially those affecting Poland. Surprising is the omission of J. E. Smith's *The Defense of Berlin* from the bibliography.

Baltimore, Maryland

ABE BORTZ

THE DIPLOMACY OF A NEW AGE: MAJOR ISSUES IN U.S. POLICY SINCE 1945. By *Dexter Perkins*. [International Studies Series.] (Bloomington: Indiana University Press. 1967. Pp. 190. \$5.75.)

THIS brief volume presents the Indiana University Patten Lectures of 1966. Its lucid if highly condensed factual narrative also provides a vehicle for the distilled views of a seasoned observer of the diplomatic scene. Succeeding chapters describe the rise of Russian-American antagonisms in Europe, resulting in stalemate and

the measures embodied in the Truman Doctrine, the Marshall Plan, and NATO, which raised the continuing specter of a violent confrontation that failed to materialize through a combination of American atomic superiority and innate Russian caution—an attribute repeatedly emphasized. Discussion of Far Eastern and Latin American matters and a final overview complete the work.

Professor Perkins finds the “salient fact” of the period to be American abandonment of isolation for deep involvement and the parallel development of unmatched military and economic power. He asks, and attempts to answer, the crucial question of the uses to which that power has been put. The cynic will at first find comfort in his frequent use of such terms as “sentimental,” “ideological,” and “emotional” in describing national motivation. This satisfaction will in the long run wither before the author’s essentially optimistic but by no means complacent attitude toward national aims and accomplishments. No economic determinist (save perhaps regarding Latin America), he sees his country as having pursued with fair success a policy of enlightened self-interest.

There is here no dearth of value judgments with which many readers may disagree; none, however, can gainsay the ripeness of research and observation from which they stem. The querulous might question, in light of other aspects of the Washington negotiations, whether “the Washington and London naval treaties of 1922 and 1930 seemed to signalize our withdrawal from the Orient”; our British cousins, contemplating the War of 1812, might raise an eyebrow on reading that the Korean War was the first in which the United States “did not attain complete victory”; and a curious mechanical aberration consistently retards the index by two pages from its textual base. This, however, is a book to be read as it was written—a short but perceptive survey of a period presently lending itself better to tentative than to firm conclusions.

Seton Hall University

L. ETHAN ELLIS

CANADA AND “IMPERIAL DEFENSE”: A STUDY OF THE ORIGINS OF THE BRITISH COMMONWEALTH’S DEFENSE ORGANIZATION, 1867–1919. By *Richard A. Preston*. [Duke University Commonwealth-Studies Center, Publication Number 29.] (Durham, N. C.: Duke University Press for the Center. 1967. Pp. xxi, 576. \$12.50.)

THIS book is the most wide-ranging, thorough, and substantial study of the defense aspect of the history of the Commonwealth yet to appear. One of its outstanding qualities is the author’s mastery of the technical aspects of defense organization, especially on the army side, and of their historical evolution.

The book is also distinguished by the scope of its research. Though the main emphasis is on Canada, as the title indicates, the book ranges widely over the Commonwealth. Substantial use has been made of the archives and the library collections of private papers and published material in Canada, Britain, Australia, New Zealand, and, to some extent, South Africa. A valuable “selective bibliography” of thirty-three pages lists separately the archives, government publications, documentary collections, autobiographical works, contemporary works, and secondary sources. Conspicuous omissions from the lists are L. S. Amery’s history of the Boer War and his three-volume autobiography, *My Political Life*. The

author's mastery of his chosen field of military history is shown by his ability to make a clear and intelligible story out of vast, scattered, and confused masses of material.

The book contains an introduction and fifteen chapters. The first nine chapters, based mainly on Canadian and British sources, take the history to the Boer War. A chapter on "A Naval Mirage—Imperial Defence 1870–1897" removes the study from the shallow backwaters of colonial military history and takes it into the age of spreading industrialism, swifter communications, and great power rivalries. The next five chapters deal with the period of the emergence of the Commonwealth, 1900–1914, and a long concluding chapter, which spills over into constitutional themes less familiar to the author, carries the history to 1919 and beyond.

The book's thesis, not too clearly set forth in the introduction, is open to question. In the introduction Preston suggests that he is doing battle against history written with "a bias toward an imperialist point of view." The publisher states that the purpose of the book is to explode "the myth of imperial defence," surprisingly attributed to "confusion about the condition, nature, and terminology of Britain's defence organisation," deriving from Sir Maurice Hankey's writings which historians have "invariably tended to follow." "Imperial defence" seems to be equated by the author with the idea of "an enforced centralised military establishment." The temptation to build interpretations on the imprecise terminology of the times can lead to questionable historical generalization, as shown by two concluding statements in the author's introduction. The dominions, it is said, invented the Commonwealth's military system of defense, which "like the Commonwealth as a whole was fashioned by the people of the dominions especially the Canadians." Did Britain really have no hand in this great business, and was it really all the work of the "colonial nationalists"? An approach to the Commonwealth from the side of defense affords too narrow a basis to justify such sweeping conclusions. There is more to be said for the author's finding that the Canadian defense role was "positive," and not largely "destructive and negative," as "commonly accepted." But here the author's conclusions have to be compared with the evidence cited by Professor James Eayrs in his two volumes on the period 1919 to 1939, *In Defence of Canada*.

Bethesda, Maryland

H. DUNCAN HALL

THE OAS AND UNITED STATES FOREIGN POLICY. By *Jerome Slater*.

[Publication of the Social Science Program of the Merzhon Center for Education in National Security of the Ohio State University.] ([Columbus:] Ohio State University Press. 1967. Pp. viii, 315. \$6.00.)

THE reader is informed on the third page that ". . . rigorous political analyses of the Organization of American States (OAS) have been meager. The major purpose of this study is to partially fill one of the most obvious lacunae by examining the role of the OAS in United States foreign policy from 1947 through 1964." If anything, thanks to the painstaking work of scholars such as J. Lloyd Mecham and the Thomases (Ann Van Wynen and A. J., Jr.), the OAS, especially in terms of its actual importance, has been overstudied. At this

stage political scientists could make a more valuable contribution by investigating some of the underlying forces that shape the policies advanced by the United States through the OAS: forces such as those generated by business, the military, labor, and the USIA. Furthermore, if studies of United States diplomacy are to be of genuine value, they should deal with the attitudes and reactions of the countries that are the objects of that diplomacy. One of the great strengths of Gordon Connell-Smith's *The Inter-American System* (1966) was its success in revealing something of Latin American attitudes toward the OAS. The present book is in the unilateral, ethnocentric approach, limited almost exclusively to the United States.

Despite these drawbacks and the conscious ignoring of economic factors, Slater's book makes some fresh contributions. Its explanation of how the OAS has been alternately a collective security system, an anti-Communist alliance, and an antidictatorial alliance is often quite penetrating. And the discussion of the ambivalent approach both of the United States and various Latin American countries to the principle of nonintervention, as they alternately honor and ignore this alleged keystone of the Good Neighbor policy, is developed in thoughtful manner.

The author's conviction that the United States should intervene more consistently in Latin American affairs in order to promote democracy may strike some readers as manifesting at the same time arrogance and naïveté. Slater's optimistic description of the accomplishments of United States intervention in the Dominican Republic, 1960-1964, written before the 1965 fiasco, reminds one of the glowing accounts of the Theodore Roosevelt administration, designed to show how early twentieth-century intervention was well on the way toward transforming the island republic into a show place of democratic and economic development.

Ignoring the history of consistent failure in earlier intervention attempts, Slater insists that the OAS should be used by the United States as a means of "helping the Latin Americans free themselves from the burden of corrupt, reactionary, and brutal government. . . ." One wonders how the United States would react to attempts of regional organizations to alter the actions of its government when these actions appear to be corrupt, reactionary, and brutal.

University of Notre Dame

FREDRICK B. PIKE

COLECCIÓN DE DOCUMENTOS PARA LA HISTORIA DE COLOMBIA
(ÉPOCA DE LA INDEPENDENCIA). Third Series. Compiled by *Sergio
Eliás Ortiz*. [Biblioteca de historia nacional, Volume CVII.] (Bogotá: Editorial A B C. 1966. Pp. 337.)

THIS latest volume of documents on the independence (and immediate pre-independence) period published by the *Academia Colombiana de Historia* is a useful though very miscellaneous addition to the published sources. It begins with an account of celebrations in honor of the Archbishop-Viceroy Antonio Caballero y Góngora held at Socorro in 1784 and closes with a brief report submitted to Viceroy Juan Sámano in 1819 concerning the territory of the Andequi Indians. In between are documents on the conspiratorial efforts of

Miranda and others; on the scientific activities of Francisco Antonio Zea; on the quarrels of the Patria Boba; on the "political conduct" of the secular clergy; and on political-military events of the years 1817-1820. The individual documents, or groups of documents, are presented in rough chronological order. Although their present location is not always indicated, the majority come from the *Archivo de Indias*; a few had been published before, but most had not. There is also a brief introduction and an index of personal names.

The documents vary rather widely in intrinsic importance as well as in subject matter, but almost any scholar concerned with the period covered is likely to find the volume worth consulting. For example, the documents that are presented (from the royalist side) on the capture and almost immediate loss of Portobelo and Río Hacha by the expeditionary forces of General Gregor MacGregor in 1819 contain numerous interesting new details. They include lists of prisoners, giving their national origin and religion, and correspondence relating to the mass execution of those taken at Río Hacha. Then, too, as the compiler points out in the introduction, the document on the Socorro festivities is an excellent "page of colonial folklore." Although there is much else of interest, it will not require any substantial revision of accepted interpretations

University of Florida

DAVID BUSHNELL

THE COLONIAL BACKGROUND OF MODERN BRAZIL. By *Caio Prado, Jr.* Translated from the Portuguese by *Suzette Macedo*. (Berkeley and Los Angeles: University of California Press. 1967. Pp. 530. \$11.00.)

THIS volume, a translation of the seventh edition (1963) of *Formação do Brasil Contemporâneo, Colônia* (1942), examines the Brazil of around 1800 with a view to understanding modern Brazil. By this date the colonial regime had run its course, and contemporary Brazil was beginning to take shape. Contemporary Brazil is "a combination of the colonial past . . . and the transformations that have occurred in the course of the last century and a half."

In "Meaning of Colonization" Prado differentiates between ancient trading stations and those in the New World, where the natives were unable to supply much of commercial value. Here it was necessary to produce items needed for trade. Europeans came to the tropics to organize production, but others were to supply the labor, "the foremost objective being the exploitation of natural resources . . . for the benefit of European commerce. This is the true *meaning* of tropical colonization, . . . and this explains the fundamental elements, both economic and social, of the historical formation and evolution of the American tropics."

With this foundation laid, the author examines the elements of Brazilian life at the end of the colonial era. He describes the settlement of the coast and the interior, and the characteristics of both. Mining brought the economic axis from the northern sugar plantations to the center, where it remained. The political balance was ultimately affected.

The Brazilian economy, designed to produce for Portugal the commodities it needed for consumption and trade, remained subordinated to this purpose.

The attitude this policy generated became so firmly fixed that it continued even after independence. The economy was characterized by cycles of prosperity and decline, and no stable balance was ever achieved.

Other chapters deal with races, large-scale and subsistence agriculture, mining, stock raising, social organization, administration, and social and political life. Those on stock raising and administration seem particularly valuable, for too little information on these is easily available.

The book, already a classic in its native land, is a genuine aid to understanding contemporary Brazil. It is an excellent synthesis of the elements of Brazilian life on the eve of independence, a proper background for examining the transformations that followed.

Texas Christian University

DONALD E. WORCESTER

A COLEÇÃO DA CASA DOS CONTOS DE OURO PRÊTO (DOCUMENTOS AVULSOS). By *Herculano Gomes Mathias*. [Publicações do Arquivo Nacional, First Series, Volume LVIII.] (Rio de Janeiro: Ministério da Justiça e Negócios Interiores, Arquivo Nacional. 1966. Pp. 290.)

DURING the past ten years the orientation of the *Arquivo Nacional* in Rio de Janeiro has shifted from a position that considered the storage of records as its exclusive function to an appreciation of the service that an archive can render as a resource for accredited scholars. The new approach, initiated by Dr. José Honório Rodrigues during his years as director, is supported vigorously by Dr. Pedro Moniz de Aragão, the current director, and his dedicated staff. It is unfortunate that the budget makers have not as yet grasped the significance of the new policy in terms of personnel and sophisticated equipment.

It has been a major objective of the new dispensation to identify and list the contents of major collections, and this book provides an interesting example of one of the ways in which this is being done. The story revolves around the papers accumulated in the Casa dos Contos de Ouro Prêto, a historic mansion in the mining region of Minas Gerais. In 1913, in order to prevent further deterioration, the documents were moved to Rio de Janeiro, a part being deposited in the *Biblioteca Nacional* and the remainder in the *Arquivo Nacional*. The portion allocated to the library was inventoried some twenty years ago by José Afonso Mendonça de Azevedo, but with the exception of a segment identified and listed by Mr. Flamarion de Siqueira, a member of the staff, the papers in the *Arquivo Nacional* have remained in cold storage. Some three years ago Dr. Herculano Gomes Mathias, a distinguished historiographer, became interested in the collection housed in the *Arquivo* through his acquaintance with the documents in the *Biblioteca Nacional*. At the invitation of Dr. Aragão he undertook the task of inventorying the Miscellaneous Papers (*Documentos Avulsos*) of the set in the *Arquivo*.

Working after hours and on weekends and holidays, Gomes Mathias checked more than 200,000 pieces. From them he selected what seemed to him to be particularly significant papers; arranged them in twelve groups such as material relating to the Tiradentes conspiracy, or documents referring to the construction

of churches and military quarters in eighteenth-century Minas Gerais, or papers connected with the activity of Manoel Francisco Lisboa and his son Antônio, "O Aleijadinho"; described each group briefly; and illustrated each with reproductions of key documents. This constitutes Part I of the volume. In Part II he inventories the entire collection, bundle by bundle. As he insists, the result is not a catalogue; rather, it is a road map for the use of scholars interested in working their way through this mass of material on the colonial history of Minas Gerais. He is to be commended for the production of a tool that will facilitate access to the treasures stored in the *Arquivo Nacional*.

Duke University

ALAN K. MANCHESTER

POLITICS IN BRAZIL, 1930-1964: AN EXPERIMENT IN DEMOCRACY.

By Thomas E. Skidmore. (New York: Oxford University Press. 1967. Pp. xviii, 446. \$8.75.)

THE dominant theme in Brazilian life since the Great Depression has been *getulismo*, a term derived from the name of Getulio Vargas who ruled Brazil with an iron hand from 1930 to 1945 and as democratic President from 1951 until his suicide in 1954. In the economic sense, *getulismo* might be called "developmentalist nationalism" or a program of modernization in which the Brazilian government deliberately industrialized the nation and diversified the economy. Essentially a moderate policy, it failed to please the extremes: the Right decried the overwhelming intervention in the economy, the deficit spending and inflation, and the favoritism to labor; while the Left clamored for stronger action against the foreign "imperialists," who had exploited the country's resources. The remarkable economic record of the Juscelino Kubitschek administration (1956-1960) was the climax of *getulismo*, but it also contributed to the soaring inflation of the Jânio Quadros (1961) and João Goulart (1961-1964) governments.

Getulismo has a political connotation as well: the deliberate measures of "The Chief" to control the labor movement, the Labor party's (PTB) role in the 1950 presidential victory, and Getulio's cooperation in that election with the traditional chiefs of Brazil (PSD) and the personalist leader Adhemar de Barros of São Paulo (PSP)—a coalition that prevailed until the election of Quadros. But Goulart, Getulio's recognized political heir, soon recovered control. His flirtation with the radical Left, however, provoked the military intervention of 1964. Thus, by force rather than democratic means, anti-*getulismo* won the day.

The author of this scholarly and detailed analysis of Brazilian politics has culled the voluminous literature on the subject; there are no less than ninety-four pages of footnotes and commentary on sources which, unfortunately, were placed at the end of the book. By and large, the treatment is objective and authoritative. One can question, however, the assumption that the United States' policy toward Cuba was well founded and that therefore those opposing it were pursuing a "quixotic policy toward the Communist world." Professor Skidmore's discussion of our role in the revolution of 1964 appears convincing: we were not directly involved although our indirect influence may have been a key factor, especially

in the Brazilian Higher War College. He might have added that our obsession with Cuba was perhaps indirectly responsible for the rash of military take-overs throughout Latin America in the early 1960's. Yet these debatable points should not detract from the value of this well-conceived volume.

George Washington University

MARIO RODRÍGUEZ

1931: OS TENENTES NO PODER. By *Hélio Silva*. [Documentos da história contemporânea, Volume XI-C. O Ciclo de Vargas, Volume IV.] ([Rio de Janeiro:] Civilização Brasileira. 1966. Pp. 407.)

1932: A GUERRA PAULISTA. By *Hélio Silva*. [Documentos da história contemporânea, Volume XI-D. O Ciclo de Vargas, Volume V.] ([Rio de Janeiro:] Civilização Brasileira. 1967. Pp. 398.)

THESE fourth and fifth volumes of Silva's "Vargas Cycle" follow 1922: *Sangue na areia de Copacabana* (1964), 1926: *A grande marcha* (1965), and 1930: *A Revolução traída* (1966). The author, a politically involved journalist, witnessed much that he narrates and knew many of the participants. He considers that the Vargas era began with the Copacabana revolt of 1922, led by the future *tenentes* of 1931-1932. These formed a "vanguard of revolutionary forces" whose program turned out to be ill defined, elusive, and impossible to execute. Vargas' advent to power ushered in two years of intrigue and conspiracy; the "revolution" was betrayed and could only generate counterrevolution, notably the São Paulo revolt of July-September 1932, with which Silva sympathizes as a Paulista patriot.

The series is not particularly valuable for political analysis, for eyewitness and hearsay testimony, or for narrative synthesis. Its usefulness resembles that of a rock quarry—as a source of building material. Like the rest of the series, these two volumes are prefaced by detailed chronologies of events for their respective periods (1931 deals mostly with "the case of São Paulo" from November 1930 to May 1932; 1932 covers May to October of that year), and they conclude with generous documentary appendixes. The text is virtually a day-by-day chronicle, with the author's factual, unobtrusive commentary weaving together a large corpus of documents (reports, proclamations, letters, telegrams), many of them *in extenso*. Books and newspapers are occasionally drawn upon, but most of the documents are from personal papers, mainly the "Arquivo Getúlio Vargas" with additional items from the papers of A. A. Borges de Medeiros, Osvaldo Aranha, Laudo de Camargo, and Eurico Gaspar Dutra.

Three random examples suggest the kind of disclosures a reader may expect: one is a fifteen-page transcript of assorted telephone conversations for July 11-18, 1931, mostly between persons in Rio and São Paulo, which were tapped by order of Miguel Costa, São Paulo's Secretary for Public Security, and forwarded to Vargas; the second is an unpublished letter of July 4, 1932, from Finance Minister Osvaldo Aranha to J. A. Flôres da Cunha, interventor of Rio Grande do Sul, in which Aranha confesses that he is worn out, embittered, and persecuted by factions, and he outlines his hope for a coalition of regional united fronts to ward off insurrection and warns that "we must not yield, neither I to the threats

of the left nor you to those of the right"; a third item of exceptional interest is an unpublished narrative by the Paulista writer Antônio de Alcântara Machado that reports high-level conversations and maneuvers in São Paulo in October 1932 when his state's military offensive had collapsed and he "knew São Paulo to be without a government."

So lavishly is Silva's text documented that his volumes become in effect a primary source for the period.

Yale University

RICHARD M. MORSE

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Donald E. Worcester, Texas Christian University

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* * * * *Association Notes* * * * *

AMERICAN HISTORICAL ASSOCIATION

News of the AHA Annual Meeting held in Toronto, December 28-30, 1967, will appear in the April 1968 *Review*.

The Service Center for Teachers of History has seventy-one titles in its series of bibliographic-interpretive pamphlets. The most recent publications are Louis Morton, *Writings on World War II*; John Hall Stewart, *The French Revolution: Some Trends in Historical Writing, 1945-1965*; Jack P. Greene, *The Reappraisal of the American Revolution in Recent Historical Literature*; David Healy, *Modern Imperialism: Changing Styles in Historical Interpretation*; Louis Galambos, *American Business History*; and Dewey W. Grantham, Jr., *The United States Since 1945*. Twenty-five titles are available in revised editions; other new and revised editions are in preparation.

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RECENT DEATHS

Richard K. Martin of Tarkio College died August 5, 1967.

Victor E. Pinkham of Northfield, Minnesota, died September 13.

Edwin B. Coddington died October 10, at the age of sixty-two. He was chairman of the history department at Lafayette College from 1946 until his retirement in January 1967. In 1966 he was elected president of the Pennsylvania Historical Association; he had served on the Pennsylvania Historical and Museum Commission since 1963.

E. T. Heald of Canton, Ohio, and Hartley Simpson of Franklin, New Hampshire, died recently.

COMMUNICATIONS

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

Irwin Unger's blatant misstatement of my views on American Socialism ("The New Left and American History . . .," *AHR*, LXXII [July 1967], 1237-63) requires correction both in fairness to myself and to provide a gauge of Unger's general reliability.

Unger wrote (p. 1250) that I have been "battling the prevailing view that

Socialism failed in the United States because of its inner inadequacies," and that I assert that "American Socialism did not fall; it was knocked down" by the repressive acts of the Wilson administration during World War I. In fact, in the article to which Unger referred, I wrote that Daniel Bell "is correct in seeking the reason [for the failure of American Socialism] in the nature of the movement, rather than in general conditions" (*Studies on the Left*, III [Fall 1963], 95). I also wrote in that essay that despite wartime repression and the weakening of the party organization, the Socialists came out of the war with good reason for optimism. The Socialist vote had increased substantially during the war, and in the early postwar years, I argued, there was "a widespread disillusionment with the war," and "a climate of opinion entirely consistent with Socialist analyses of the war" (*ibid.*, 99). I wrote in another piece, also cited by Unger, that "the party did not pull its scattered forces back together after the war, but I believe this was the result of the split of 1919, rather than the logical outcome of wartime isolation" (*ibid.*, IV [Summer 1964], 96).

Further on, Unger also distorted my criticism of the historians of the Socialist and Communist parties. He paraphrased me as follows: "Socialism is America's 'hidden heritage,' which has been maligned and abused as much by the historians as by the Attorney-General" (p. 1251). But I did not write nor imply that Socialism is America's hidden heritage. Indeed, I was discussing something quite different. My argument was that the heritage of American Socialism, a relevant minority movement of anticapitalist radicalism, has been obscured by a mythology created by the post-1919 Left, and that historians have reinforced that mythology for ideological reasons (*Studies on the Left*, III [Fall 1963], 108). The distinction should be clear, particularly to one who has read my essays with reasonable attentiveness. The result of Unger's distortion is to make me look a bit simple-minded. Whether he labored to do that or was only sloppy I cannot say.

San Francisco, California

JAMES WEINSTEIN

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

Professor Irwin Unger's perceptive appraisal of Left-wing historians deserves the respectful attention and appreciation of those he discusses as well as of the profession at large. Despite some unnecessary personal asides, unfair snap judgments, and too much evidence of that patronizing tone which often intrudes itself into his work, it should be recognized as an honest and thoughtful attempt to come to grips with trends too many others vainly hope will go away. Those who heard the original version read in Chicago and who heard Professor W. A. Williams' dignified and illuminating response may recall how they saved the session from qualifying as one of the most shameful exhibitions ever to disgrace one of our professional meetings, marred as it was by a second paper consisting of personal slanders, abusive language, and undisguised ideological malice and a second commentary displaying breath-taking ignorance of the New Left as well as professional irresponsibility.

Professor Unger raises too many questions for brief reply, and his particular arguments ought properly to be answered by different individuals. I should like to consider one general question and illustrate it with reference to his generous

but mistaken remarks on my work on slavery. I agree with him that a radical politics does not require repudiation of the consensus view of American history; that such a view must be examined empirically and without bias; and that present-mindedness mars much of the new Left-wing historiography, just as it marred much of the old. I see no principled objection to the consensus view in its more sophisticated versions, although if the bloody years 1861–1865 formed part of that consensus, we need a new vocabulary. In general, no Marxist ought to be embarrassed if the consensus view withstands the attacks of its critics: We should then have before us the particular history of the process by which the American bourgeoisie established its hegemony. We would not thereby be required to join the celebration. If such has been the true history of American capitalism, it would be incumbent upon radicals to study the secret of its success so as the better to oppose it. Radicals need a proper understanding of class dynamics and social forces, not a “usable past.” Even if there had never been a viable radicalism in America, only Philistines would argue that therefore there never could or should be. Professor Unger is, on these questions, wise and to the point. But for just this reason he ought to reconsider his notion that *The Contours of American History*, whatever its specific merits or demerits, “gives aid and comfort to the enemy.”

Professor Williams in his way and others (by no means all) in theirs are trying to interpret American history in class terms. We know very well that a class interpretation does not imply an emphasis on class confrontation and struggle; it may reveal a process by which a given ruling class successfully avoided such confrontations. It was Marx himself, after all, who first pointed out that Asia had nothing like a social revolution until the advent of imperialist penetration. Professor Unger, therefore, makes too sharp a distinction between the Old and New Left: both have had a tendency to exaggerate confrontation and have, accordingly, failed to see the processes by which ruling classes may avoid such confrontation. It is no accident that such striking problems as the loyalty of the yeomen to the slave regime have received virtually no attention from the Left.

Contrary to Professor Unger, I should be prepared to argue—and have argued in detail in an essay to appear soon in Barton J. Bernstein’s *Towards a New Past*—that Marxists, beginning with Marx, have made a mess of their interpretation of the Old South and of the origins of the war. Two strains in Marxist and non-Marxist radical historiography account for this: the crude economic determinism into which even Marx and Engels sometimes retreated, and the tendency to adjust historical analysis to immediate political pressures. These, contrary to Professor Unger’s implications, have plagued the historiography of the Left as a whole (and not only the Left!) for a century. The philosophical idealism of some of the New Left historians has not enabled them to correct these weaknesses; it has at best led them to repeat past mistakes and at worst to compound them.

The best of the new radical scholarship is groping toward a class interpretation, as Professor Unger shrewdly observes, but for just that reason it marks the first stage of a resurgent Marxism. It forms a counterpart to the reassertion of an undogmatic Marxism in Europe and will, sooner or later, have to wed itself to the Neo-Marxism most closely associated with the work of Antonio Gramsci. To Marxists being raised in this spirit, a class interpretation is at once economic, political, ideological, and moral; it is, by its very nature, opposed to economic

determinism, which it regards as being not only wrong but silly, and in fact to determinism in general, except in a special sense of the word.

Let me illustrate the point with reference to Professor Unger's treatment of my essays on slavery and the War for Southern Independence, which, being a Yankee, he insists on calling a "civil war." Professor Unger sums up my viewpoint as follows: "Slavery, at least by implication, caused the war, not because it aroused the moral indignation of the Western world, but because it isolated the South from the progressive economic currents of the day." He defends me against the charge of economic determinism—for which I am grateful—but nonetheless misses the point. I never intended to slight the moral question; without it there probably would have been no war, however sharp the political and economic antagonism. My point, at the risk of oversimplification, is that the South and North developed different systems of morality just as they developed different systems of economy and that they did both because the classes rising to power in each section diverged fundamentally, that is, diverged in their elemental relationship to other human beings. For this reason I stressed the total antagonism of the free and slave labor systems. I suggest a moral relativism here, but only on one level of historical analysis. In doing so, nothing prevents me from taking sides politically, in part as a response to the moral question. In arguing that the moral defense of slavery was an authentic and legitimate expression of the world view of its ruling class, and not a vulgar rationalization, I say that it therefore deserves to be treated with care, respect, and seriousness; I do not argue that the issue of North versus South did not present itself in part as a fundamental moral antagonism—quite the contrary—and I do not argue that the resolution of that moral antagonism was a matter of little or no importance for the development of modern civilization. Surely, Professor Unger, of all people, knows that an attempt at sympathetic understanding does not imply approval or moral neutrality. (If he will permit me to lecture him on one point: in irreconcilable confrontations, as Comrade Stalin, who remains dear to some of us, clearly understood, it is precisely the most admirable, manly, principled, and by their own lights moral opponents who have to be killed; the others can be frightened or bought.)

Were Professor Unger to review his essay, he would have to reconsider the link between the old radical historiography and the new, but more important, he would have to reconsider future prospects. The class analysis he sees emerging and the greater restraint and objectivity for which he pleads are likely to converge in a way his essay fails to notice: in the re-emergence of a Marxism that is purged of its economic determinism and its naïve romanticization of oppressed classes and peoples—a Marxism capable of doing for the United States what Lenin, Gramsci, and Mao, in radically different ways, did for their respective countries.

Sir George Williams University

EUGENE D. GENOVESE

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

I am not certain that I understand what Mr. Weinstein is so annoyed about. To begin with, I specifically exempted him from simple-mindedness in a note in my article on page 1251. But beyond this I insist that I have not distorted Weinstein's view that Socialism failed in America because of the external circumstances of its

environment and the truculence of its opponents rather than its own inadequacies. In his reply to Professor Gerald Friedberg's article, Weinstein, after describing how popular and successful was the antiwar stand of the Socialist party of America, then goes on to note (*Studies on the Left*, IV [Summer 1964], 94-95) that a campaign of federal repression "that makes the recent McCarthy days appear to be models of polite political toleration" destroyed much of the strength the party had built up particularly among native Americans. By November 1918, he concludes, "Socialists had functioning organizations in only a few areas. . . ." Weinstein, it is true, then notes that all chance of subsequent Socialist recovery was lost owing to the divisive effects of Bolshevism rather than federal repression, but this too, after all, represents a shift of blame for failure from the Socialists themselves to their opponents. In my further defense I might add that Professor Friedberg's conclusions about what Weinstein is saying are almost identical to mine.

I consider Professor Genovese's letter an intelligent Marxist's plea for intelligent Marxism. I approve of almost all of it and would be immensely pleased to see Genovese and his colleagues carry out the program he calls for. But I do demur at one point. Professor Genovese in his first paragraph disapproves of what he considers some gratuitous *ad hominem* remarks by me. In the same paragraph, however, he makes some himself about two of the speakers at the session of the Organization of American Historians in Chicago where my article was first given as a paper. I wonder why Professor Genovese has not also felt called on to impugn the discernment and the manners of some of the more contentious New Left historians—Mr. Weinstein, for example.

New York University

IRWIN UNGER

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

As a nonprofessional historian with a passion for Hawaiian history, I am pleased with the review by Professor Harold Whitman Bradley of my book on Claus Spreckels (*AHR*, LXXII [July 1967], 1503).

I have a minor complaint. Professor Bradley writes: "Despite careful research, the author has uncovered little previously unknown concerning the Hawaiian career of Spreckels." I claim little that is new as to the facts of Spreckels' Hawaiian career, but I do claim that I am the first to put these facts together in a way that clearly shows his overwhelming importance in the late nineteenth-century economic development of Hawaii.

University of Hawaii

JACOB ADLER

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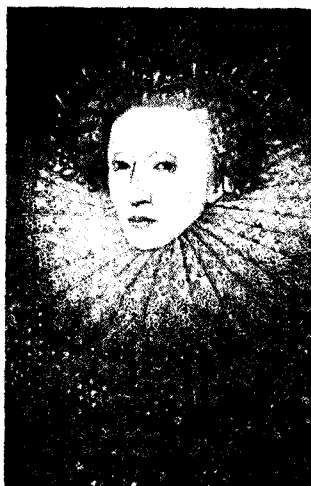
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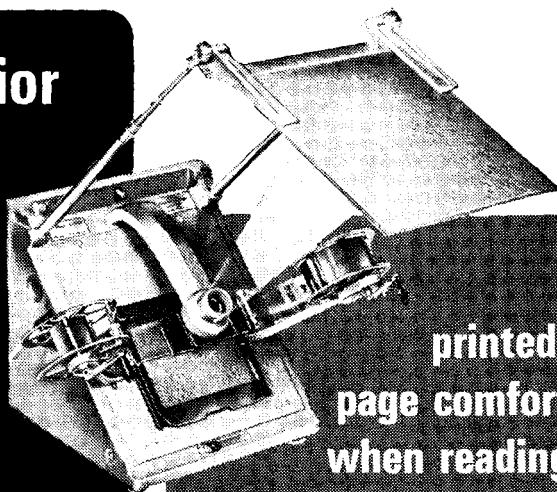
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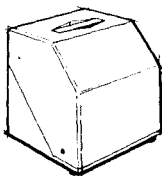
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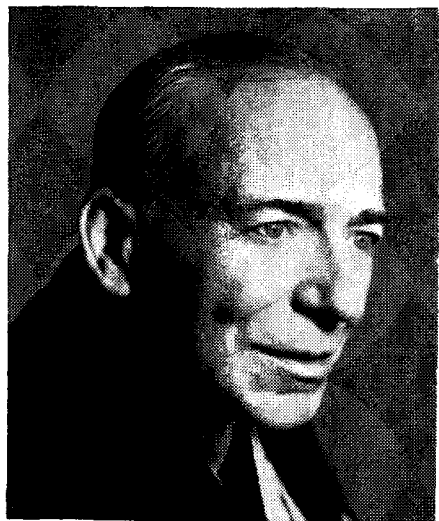


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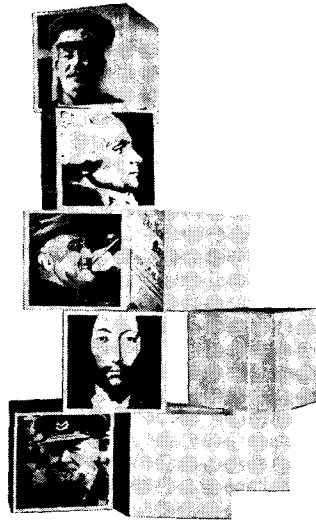
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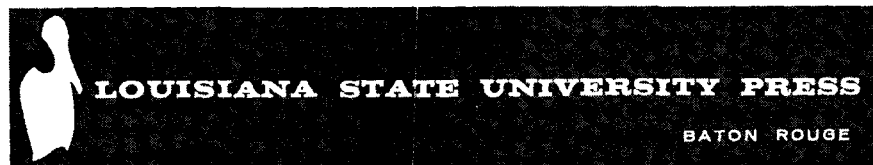
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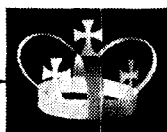
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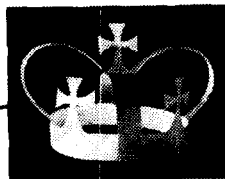
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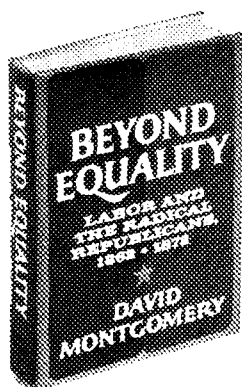
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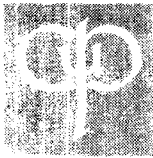
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